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THE
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PSYCHOLOGY

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PSYCHOLOGY OF ANIMISM.

BY CARVETH READ.

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I. *What is Animism?*

IN anthropology, the term Animism is usually employed to denote the proneness of savages and barbarians, or people of unscientific culture, to explain natural occurrences, at least the more remarkable or interesting—the weather, the growth of crops, disease and death—as due to the action of spirits: (1) ghosts (that is, spirits that have formerly been incarnate); (2) dream-spirits, that have temporarily quitted some body during sleep or trance; (3) invisible, living, conscious beings that have never been incarnate. This I propose to call Hyperphysical Animism. Sometimes, however, 'Animism' is used to denote a supposed attitude of savages and children toward all things, animate and inanimate, such that they spontaneously and necessarily attribute to everything a consciousness like our own, and regard all the actions and reactions of natural objects as voluntary and purposive. And this I propose to call Psychological Animism. These two meanings of the word are entirely different: it is one thing to regard an object as moved by its own mind, another to attribute its movement or influence to a separable agent which for the time possesses it; it is one thing to regard an object as having an anthropomorphic consciousness, another to believe that that consciousness is a distinct power capable of quitting it and sometimes returning, or of surviving its destruction, or of existing independently. Even if the doctrine of Animism in the second sense were granted, it would remain to be shown how men came to conceive that the consciousness of a thing can be separated from it, and exist and act by itself, and even with greater powers than it had before—contrary to the opinion of Don Juan,

> that soul and body, on the whole,
> Were odds against a disembodied soul.

Savages do not always regard a separable spirit as necessarily belonging even to human nature. Dr Seligman writes that, among the Veddas, a few old men "were by no means confident that all men on their death became *yaku*." Influential men and mediums would do so; but for the rest, at Godatalawa it was determined by experiment. The ordinary man was invoked soon after death, and desired to give good success in hunting; and if much game was then obtained, he had become a *yaka*[1]. Colonel C. H. Stigand says "the Masai have no belief in a future state for any but chiefs"; the common dead are not even buried, but merely thrown out into the bush[2]. Among the Omaha, though each person has a spirit that normally survives the body, still, a suicide ceases to exist[3]. The human spirit, then, is not necessarily believed to enter upon a life after death; still less is the spirit of an animal. On the other hand, it may be held that something inherent in weapons, utensils, food and other objects, a 'soul' or soul-stuff, may be separable from them and go to Hades or serve as the food of spirits, although the things themselves are not regarded as having a spirit or intelligent life.

II. *Psychological Animism.*

Andrew Lang described savages as existing in "a confused frame of mind to which all things, animate and inanimate,...seem on the same level of life, passion and reason[4]." Children and other immature people are often supposed to be in the same condition. As to children, it is pointed out how deeply concerned they are about dolls and rocking-horses, how passionately they turn to strike a table after knocking their heads against it. But probably it is now admitted that impulsive retaliation, on a table or bramble or shirt-stud (not unknown to civilised men), implies not any belief in the malignity or sensitiveness of those objects. Moreover, in children, such behaviour is in large measure due to suggestion; inasmuch as the setting of them to beat the table, or what not, is an easy way of diverting them from their own pain. And, of course, the dealing with dolls, or rocking-horses, or walking-sticks, as if alive, is play. Such play involves intense imaginative belief, which, at first, is not differentiated from earnest. But this stage corresponds with the play of the young of the higher animals, whilst they are still physically incapable of completing the preluded actions. By the time that children are at all comparable with savages, their play has become a temporary attitude, compatible with brusque transition to matter-of-fact, or even with actions which at the height of play show that the illusion is incomplete.

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[1] *The Veddas*, 126–7.

[2] *The Land of Zing*, 219

[3] J. O. Dorsey, *American Bureau of Ethnology*, 1889–90, XI. 419.

[4] *Myth, Ritual and Religion*, 48.

In savages, likewise, much of the behaviour that is supposed to betray an illusionary animism, even in their simple apprehension of things, is really an acquired way of acting, in a temporary attitude, under the influence of imagination-belief, and is compatible with other actions that show how incomplete is the illusion. Andrew Lang, after the passage above quoted, appears to limit the scope of it by the words "when myth-making": no doubt, when myth-making and in practising many rites, savages speak or act as if they believed in the full sense that the objects dealt with are sensitive intelligent beings; and yet their effective conduct toward them is entirely positive. They may, for example, feed the growing rice-plant with pap; in harvesting it, speak a secret language that the rice may not understand them and be alarmed, and proceed to cut it with knives concealed in their palms: but they do cut it. They carry it home and garner it with honour, and come from time to time to take a portion for food with solemn observances: but then they cook and eat it[1]. Their animistic attitude, therefore, is not primitive, spontaneous, necessary illusion, but an acquired, specialised way of imagining and dealing with certain things. Were it not possible to combine in this way the imaginative with the practical, all wizardry and priestcraft would be nothing but the sheer cheating which it often seems to be to superficial observers. Normally, imagination-beliefs are unable to overcome biological needs; but often they do so within certain limits, or in certain directions, as in innumerable taboos of food, customs of destroying property, starving or maiming tribesmen on the war-path. An universal taboo on rice is not inconceivable. For these are social-pathological cases; like the self-destructive beliefs of individuals and coteries amongst ourselves, such as the faith-healers, who in sickness call upon their god instead of a doctor.

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[1] Frazer, *Spirits of the Corn and the Wild*, I. 183.

Children, savages and ourselves, in some degree, attribute spontaneously to inanimate things, in our mere apprehension of them (for this has nothing to do with the metaphysics of Pampsychism), something more than external existence: regarding them as force-things and, by empathy, as experiencing effort and quiescence, strain and relief, and sometimes emotion and pain. It is for this attitude toward nature that I adopt Mr Marett's term 'animatism': as not ascribing to inanimate things, or to plants, in general, anything like a human personal consciousness; but merely an obscure, fragmentary, partial consciousness, enough to correspond with our occasional experiences in dealing with them. Perhaps those observers who report in strong terms universal Animism as the tenet of a tribe, mean no more than this; for example, the author above quoted as writing in the *American Bureau of Ethnology*, who says (p. 433) that according to the Dakotas, everything—"the commonest sticks and clays"—has a spirit that may hurt or help and is, therefore, to be propitiated. Probably it would be unjust to the adherents of Animism to accuse them of believing that savages have universally made so much progress in "faculty Psychology" as to distinguish personality, will, passion and reason; especially as they add that savages project these powers into all natural objects through incapacity for discrimination and abstraction; and, at the same time, know very well that in some languages of the most animistic tribes (Algonquin and Nága) the distinction of animate and inanimate is the ground of grammatical gender.

We find, accordingly, that some explorers explicitly deny that, in their experience, savages regard all things as on the same level of life, passion and reason. Dr Coddrington says that, in the Banks' Islands, yams and such things are not believed to have any *tarunga* (spirit); "they do not live with any kind of intelligence[1]." Messrs Skeat and Blagden report that with the Semang of the Malay Peninsula there is very little trace of animistic beliefs; and they relate a folk-tale of how a male elephant tells a female that he has found a live stone (pangolin rolled into a ball): "Swine," said the female, "stones are never alive[1]." Messrs Hose and McDougall tell us that the Kayans hang garments and weapons on a tomb, and seem to believe that shadowy duplicates of these things are at the service of the ghost, but that such duplicates are inert (relatively) and not to be confused with the principle of intelligence[2].

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[1] *The Melanesians*, 249.

To be clear about Animism, it is necessary to bear in mind several modes of belief: (1) Hyperphysical Animism, that things have, or are possessed by a conscious spirit, and that this spirit is a separable entity; (2) that things are themselves conscious, but their consciousness is not a separable entity; (3) that things are not conscious, but are informed by a separable essence, usually called soul (better, soul-stuff), which may be eaten by spirits, or may go to ghost-land with them; (4) the extension or limitation of these beliefs to more or fewer classes of things. Unless these distinctions are recognised, any report upon savage beliefs can hardly be clear and adequate; but generally we may take it that when a traveller tells us that such and such things are not believed to have souls, and says nothing of any belief as to their consciousness, he means (except with regard to animals) to deny that anything like human consciousness is attributed to them. Thus when Mr Torday writes that, according to the Bahuana of the Upper Congo, there are two incorporeal parts—*doshi*, common to man, animals and fetiches, and *bun*, peculiar to man—he seems to leave it as a matter of course that plants and inanimate things have neither of these, and are not conscious beings; though probably some of them have soul-stuff, since clothes, weapons and food are buried with a corpse[3]. The Rev. J. H. Weeks says that the Bakongo of the Lower Congo attribute a spirit only to the *nkasa* tree (from whose bark the ordeal poison is derived) amongst plants[4]; and, similarly, the Baloki, further up the river, attribute a spirit only to the *nka* tree[5]. Since many things are buried by these people in a grave, or broken above it, the things must be supposed to have soul-stuff; but from the denying of spirits to plants, and from silence as to psychological Animism, it may be inferred that neither plants nor inanimate things are regarded as conscious beings. Sir E. F. im Thurn tells us that material things of all sorts are believed by the natives of Guiana to have each a body and a spirit—evidently a conscious and malicious spirit; "and that not all inanimate objects have this dual nature avowedly attributed to them, is probably only due to the chance that...the spirit has not yet been noticed in some cases[1]." Even with these Guiana Indians, then, whose Animism, in every sense is unusually active and extensive, their attitude is an acquired, specialised way of imagining and dealing with things that draw their attention and excite their suspicions, not a primitive, necessary illusion; else there could be no exceptions.

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[1] *Pagan Races of the Malay Peninsula*, II. 222.

[2] *Pagan Tribes of Borneo*, II. 3.

[3] *Camp and Tramp in African Wilds*, 174.

[4] *The Primitive Bakongo*, 283

[5] *Among Congo Cannibals*, 275.

The reasonable view, then, is that savages distinguish between themselves and certain animals, on the one hand, and, on the other, the remaining animals, plants and inanimate things; and raise the second class to the rank of the first, as conscious agents, only when there are special incentives to do so. To find all the causes that excite the animistic attitude toward things would be a difficult task, but some of them may be indicated. Beginning from Animatism, which really is a primitive and necessary illusion, it is reasonable to expect:

(*a*) That any plant or inanimate thing adopted as a totem should, by that very fact, be endowed with human consciousness; though the savage mind is too inconsistent for us to infer that this must always happen.

(*b*) That whatever seems to move or act spontaneously, like the winds and streams and echoes, the sun, moon, planets, and shooting stars, should be felt as a spiritual agency, especially if it cry out with empathetic reverberation, as the winds and cataracts do, the waves, fire, and the ice-floe when it breaks up in spring; or if it be extraordinary and dangerous, as the thunder and lightning, whirlwinds and whirlpools are, waterspouts and volcanoes. A whirlpool that has drowned a man, thenceforth is, or has, a spirit. As to waterspouts and shooting stars, see the *Reports of the Cambridge Expedition to Torres Straits*, (VI. 252). As to volcanoes, Dr Speisser writes that at Ambrym, when the volcano is active, the natives climb to the top and bring sacrifices to appease it, throwing coconuts and yams into the crater[2].

(*c*) That whatever has been regarded as having magical force should be treated—after the rise of the ghost-theory (supposing this to be of later origin) has given vogue to a new principle of explanation—as owing its virtue to a spirit, either by immanence or possession (two modes of actuation which may or may not be distinguished), and so become a fetich.

(*d*) That whatever is much used in ritual, especially if often addressed in spells or incantations, should become an object of reverence, apt to be personified and raised to, or even above, the human level as a conscious agent: for example, padi and rice in Indonesia, the ordeal tree on the Congo, already mentioned. In India, the conch, having for ages been used in religious rites, "the people gradually came to revere the instrument itself and to adore and invoke it[1]." Amongst the very few inanimate gods of the Cherokees are the Stone, invoked by the Shaman when seeking lost goods by means of a pebble suspended by a string; and the Flint, invoked when about to scarify a patient with an arrow-head before rubbing in medicine[2]. By the Apache, heddontin (pollen of the cat-tail rush) is used as the sacrificial powder in nearly all rites, and is personified and prayed to[3].

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[1] *Among the Indians of Guiana*, 355.

[2] *Two years with the Natives of the West Pacific*, 199.

When spells are addressed to any object, the analogy of address to human beings causes that object to be thought of as humanly conscious. Stocks and stones have been worshipped, and regarded as the dwelling place of spirits in most parts of the world; having superseded in the mind of their devotees the ghost of the man whose burial place they formerly marked, and where offerings were made.

(*e*) Where Animism is active amongst a timid and suspicious people, whatever injures a man is believed to act of malice: as amongst the Indians of Guiana, who are so timid that rather than go hunting alone they will take a woman or a child along with them[4].

Under such conditions as these psychological Animism is very widely though very irregularly diffused; but were it universal and uniform, it could not of itself account for hyperphysical Animism—the doctrine that men (or some men), some animals, plants, things, places, are possessed or informed by spirits that are capable of separate existence.

III. *The Ghost Theory.*

Hyperphysical Animism may be easiest understood as having arisen with the belief in human ghosts. The causes of this belief have been fully set forth by Herbert Spencer and Sir E. B. Tylor in a way that to my mind is convincing. Amongst those causes dreams predominate; wherein the dead are met again as in the flesh. The living body having always been for the savage a conscious force-thing, at

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[1] J. Hornell in the *Quar. J. of the Mythic. Soc.* (Bangalore), IV. No. 4, 158.

[2] J. Mooney in *Reports of Am. Bureau of Ethn.* 1885–6, VII. 341.

[3] J. E. Bourke in the *Reports of Am. Bureau of Ethn.* 1887–8, IX. 499–507

[4] E. F. im Thurn, *Among the Indians of Guiana*, 238, 354.

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death the conscious force leaves the thing or corpse. This might be accepted by him as a fact of the same kind as the loss of its virtue by a talisman or amulet (which is known sometimes to happen), were it not for dreams in which the dead still live. That this conscious force that has left the body is not visible except in dreams need excite little wonder, since many forces natural and magical are invisible.

A dream not being common to two men at the same time, the things that are seen in it cannot be pointed out, nor therefore directly named (in this resembling subjective experiences); so that to find names with which to describe such things men turn to other ways in which they seem to have a double existence, to shadows and reflections: which in their sudden appearance and disappearance, and sometimes faint, sometimes distorted outlines, bear some resemblance to dream-images; and probably it is felt to be significant that shadows and reflections disappear at night, just when dreams occur. Shadows and reflections are not necessarily identified with the ghost derived from the dream-image, because their names are given to it; but sometimes they certainly are; so that a man who, on looking into water, happens not to see his reflection may believe that his spirit has gone away, and that he himself must be ill, and accordingly he becomes ill; or if at noon near the equator he notices that he has no shadow, he may run to a medicine-man to get it back; and dead bodies may be believed to have no shadows[1]. Inasmuch as a body even lying on the ground casts a shadow, except at tropical noon, the belief that it does not do so at any time implies an acquired inability to see what is before one's eyes; as sometimes happens in hypnosis and other conditions of negative hallucination.

The idea of a separable conscious personal force, or spirit, that leaves the body at death, serves also, as it gathers strength, to explain sleep, fainting, epilepsy; and sometimes every sickness is attributed to the partial detachment or desertion of the spirit; which, therefore, it is the doctor's business to plug in, or to catch and restore; or (if I rightly remember a report of Miss Kingsley's) he may even supply another one from a basket of souls kept at hand for such exigencies.

That the ghost theory arises not only from dreams, but is also suggested by hallucinations and hypnagogic visions is very probable. In various parts of the world savages have been described as having visions of remarkably coherent and convincing vividness that seem not to have been dreams[1]. But such experiences, even when artificially induced by fasting or drugs, as happens among many tribes, are rare in comparison with dreams; and to the influence of dreams upon these savage beliefs there is abundant testimony. With some tribes dreams are treated as part of their objective experience; so that to be injured by your neighbour in a dream is just ground for avenging yourself as soon as you wake; and to see a dead man in a dream is, therefore, clear proof of his continued existence, and that either he has come to the dreamer or the dreamer has gone to visit him. It is recognised by psychologists that dreams, as immediate experience, have more the character of perception than of imagination. Children are apt to confuse dreams with reality. It can only have been gradually, with the growing knowledge of continuity and coherence in the course of events, and therewith the demand for corroboration of testimony, that dreams were distinguished from the waking life. When no longer supposed to be all of them real, some are still so regarded: the Dieri, amongst lower savages, distinguish between visions, as revelations made by Kutchi (the Evil Spirit), and ordinary dreams, as mere fancies[2]. But so impressive are dreams to many people, in their eagerness to know more than sense and philosophy can tell them, that they persist in hoping, and therefore believing, that dreams, if they give no knowledge of this world, may still be revelations of another, perhaps more real; or if not revelations, adumbrations by way of allegory, which some learned or inspired Daniel may interpret; or, at least, omens of good or evil, which the ancient science of Oneiromancy undertakes to explain.

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[1] J. H. Weeks, *Among Congo Cannibals*, 262–3.

That the dead are seen alive in dreams is, then, for the savage a fact of observation; and, therefore, the continued existence of the dead is, for him, not in the first place supernatural; although it may be called hyperphysical, because it is experienced only in dreams and not by daylight, and is exempt from ordinary conditions of time and place. But it gradually becomes supernatural, as the capricious incidents of dream-life are felt to be 'uncanny,' as that which occurs only at night is involved in the fears of the night, and as a great cloud of imaginations accumulates about the dead and obscures the simple facts of dream-perception in which the belief originated. This cloud of imaginations, by its mysterious character and by various alliances with Magic, spreads and deepens until it overshadows the whole of human life; is generally, indeed, dispersed here and there by the forces of biological necessity, often by subterfuges laughable enough; which have, however, the merit of saving mankind from destruction: but sometimes it extinguishes the last ray of common sense, impoverishes the believer, enfeebles him, fills his days and nights with terror, gives him over to practices the most cruel or the most disgusting, leads him to slay his own tribesmen, his own children, his own parents, and to offer up himself in the sure hope of resurrection.

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[1] See e.g. A. W. Howitt, *Nat. Tribes of S.E. Aust.*, 406; and P. A. Talbot, *In the Shadow of the Bush*, 83–8.

[2] A. W. Howitt, *op. cit.* p. 358.

IV. *Extension of the Ghost Theory to Animals.*

Spirits, having once been conceived of as explaining the actions of men and surviving their bodily death, may by analogy be conceived to explain the action of any other things in circumstances that suggest a motive for the action, and therefore to possess or inhabit such things, and to be capable of separating from them, like ghosts. Other things are already, by Magic and Animatism, force-things, in some degree conscious, whose forces may be capable of acting invisibly at a distance; and at the death of a man it is his conscious force that leaves the body and becomes a ghost. Since, then, there is hardly any natural object whose action may not in some circumstances seem to be interpretable by motives, especially amongst a timid and suspicious people, how can we assign any necessary limits to the spread of Animism? Moreover, the causes most influential in establishing the ghost theory for man directly require its extension to other things. For not human beings only are seen in dreams, but also their weapons and utensils, and also animals, plants, localities. If, then, the dead, because they are seen in dreams, are inferred still to live under conditions in which they are not visible by daylight to ordinary men, how can the inference be avoided that all sorts of things, artefacts, animals, plants, localities, share in that mode of existence—that all have their doubles? And "Why not?" the savage might ask, since it is literally true of all things, without exception, that they are sometimes visible, sometimes invisible. Similar inferences seem to be justifiable from the alliance of ghosts with shadows and reflections, and the fact that not man only but everything else has a shadow and a reflection, and that their shadows and reflections disappear at night, just when the things themselves sometimes appear in dreams. Moreover, so far as the breath, the pulse, the shining of the eye, which cease in the human corpse, are sometimes identified with the departed spirit, the same processes likewise cease at the death of animals; though, it is true, there is here no analogy with inanimate things, and the breathing and circulation of plants are beyond the savage's observation. Therefore, although Animism is an inferential construction, were the construction entirely due to the logic of analogy, there would be nothing surprising in the discovery that the belief 'that everything has a ghost' is just as universal and uniform in the human race as if it had been an innate or primitive belief. That, on the contrary, Animism prevails very irregularly amongst the tribes of men; that, in all directions, inferences that are analogically specious fail to be drawn; that instead of a general theory of Animism every tribe has its own Animism; this is surprising and needs to be explained. The extension of the theory is easier to understand than its irregular limitation.

Bearing in mind that we are at present considering Animism as a belief in ghosts, not in spirits generally (to which we shall come in Sect. VI), I venture to think that, although dreams, shadows and reflections certainly suggest a double existence of everything, yet savages never assign a true ghost to anything inanimate, nor to plants, nor even to animals, unless there is a special reason for doing so; because only in the case of human beings is the suggestion interesting enough to take hold of the imagination. Hence, even though other things appear in the ghost-world, they have no significance there, except in relation to human ghosts (or ghostlike spirits) on whom they attend. Accordingly, human ghosts have a place in the beliefs of every tribe, because human beings excite affection, admiration and fear, have well-marked individuality; are therefore remembered, have stories told of them; and if they are seen after death, it is, of course, reported. The evidence makes it only too plain that the paralysis of attention by fear is the chief (though not the only) emotional factor of belief in ghosts; and what other thing in all nature is to be feared in comparison with one's fellow-man?

The belief in ghosts, escaped and roaming independent of any normally visible body, as a social belief, is involved in the practice of reporting and discussing dreams, which becomes the same thing as telling ghost-stories—the first and most persistent motive of literature. Stories can only be told effectively of things generally interesting; and, at first, such things must have been recognisable by the hearers and must have had some individuality. Hence—

(*a*) Animals that attain to such individuality may have ghosts: (i) An animal that occasions widespread fear, such as a man-eating

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tiger. We must distinguish from such cases the frequent beliefs that tigers, wolves, sharks, snakes, etc., are, or are possessed by, the spirits or ghosts of men. (ii) An animal that comes to be upon terms of special intimacy with men; such as the very tame dogs and pigs that come when they are called among the Bakongo[1]; or hunting dogs that have been specially doctored among the Baloki[2].

(*b*) Animals slain at funeral feasts to accompany the dead have ghosts so far as necessary for that purpose; but, wanting individuality and personal interest, they make no further figure in ghost-lore; they do not 'walk' or revisit the glimpses of the moon. Thus the Tanghouls say that ghosts, on reaching Kazairam (their Hades), find the gates barred against them by the deity Kokto; so at the burial feast of a rich man a buffalo is killed that his mighty ghost may burst open the massive gates of that abode. Poor ghosts must wait about outside, till a rich one comes up with his buffalo; when they all rush in behind him[3]. But we hear nothing further of the buffalo. In the Banks' Islands, pigs killed at a funeral feast have no true ghosts to follow the dead to Panoi, but only a sort of wraith; because they only go for show, that their master may be well received there[4].

(*c*) Animals that are important prey to a hunting tribe are often believed to have ghosts that may be hunted by dead tribesmen; or that must be propitiated when one of them is slain: the ghosts, for example, of seals and bears are bribed by the Esquimo to entice other seals and bears to come and be killed. A seal desires above everything, they say, a drink of fresh water; so as soon as one is brought ashore a dipperful is poured into his mouth; else the other seals will not allow themselves to be caught. The polar bear (male) desires crooked knives and bow-drills, or (female) women's knives and needle-cases. Hence, when a bear is killed, its ghost accompanies its skin into the hunter's hut; and the skin is hung up with the appropriate tools for four or five days. Then the bear-ghost is driven out by a magic formula, takes with it the souls of the tools, and reports well of the hunter in bear-soul land. Whilst in the hut, as an honoured guest, nothing is done that it dislikes in human customs[5].

(*d*) In the development of mythology, animals and monsters of various kinds may be found inhabiting shadow-land; but these are not true ghosts of any particular things that once died in this world.

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[1] J. H. Weeks, *The Primitive Bakongo*, 238.

[2] J. H. Weeks, *Among Congo Cannibals*, 233.

[3] T. C. Hodson, *The Naga Tribes of Manipur*, 160.

[4] Coddrington, *The Melanesians*, 269.

[5] V. Stefànson, *My Life with the Eskimo*, 57.

This list of the ways in which animals may come to have ghosts is not offered as exhausting all the cases.

V. *Ghosts and Soul-stuff.*

A ghost is a disembodied soul, having a consciousness and power at, or (generally) above, the human level; but there may be disembodied souls, or souls capable of disembodiment, that have no consciousness, or none above the level of Animatism. Even if a living thing have a consciousness, its post-mortem apparition may not; like the Banks' Islanders' pig, which, though "a distinguished animal and acknowledged to be intelligent," has no true ghost. Among nearly all tribes, whatever is offered in sacrifice to gods or left in, or at, the tombs of men deceased, is believed to have some sort of soul; because, plainly, spirits do not eat or consume the visible food or utensils; yet it is necessary to the success of the rites to suppose that the spirits are satisfied; they must, therefore, take the souls of the offerings. And what can be more plausible reasoning than to argue that, as solid men eat solid food, ghosts eat ghostly food. 'Soul' thus appears as a sort of ghost-substance, or ghost-body. For, in dreams, the departed are seen as if in the flesh; and moreover analogy requires that the ghost consciousness and ghost-force shall have a body of some sort, and, of course, one that will maintain in ghost-land the same relations to other things that the mortal body did in this world. In ghost-land, or shadow-land, or dream-land, the substance of all things is this soul-stuff. Sometimes the force of analogy requires a tribe to believe that, in order that the souls of things may be released to accompany a ghost to the underworld, the things themselves must be "killed," that is to say, broken; but other tribes are not such consistent reasoners; and in some cases where things left exposed at a grave (not buried) are broken, it may be to prevent their being stolen.

Anything, then, may have soul after its kind: relatively inert things have relatively inert souls, but never true ghosts; some animals may have ghosts, especially if they have attained to a certain individuality, but generally only in so far as they are imagined to attend upon human ghosts or spirits. Inasmuch as the word 'soul' is often used as equivalent to 'ghost,' it would be convenient always to speak of the soul which is ghost-food, or ghost-body, as soul-stuff[1]. Soul-stuff is conceived of as material, though subtle and normally invisible. A man's soul-stuff may be regarded not only as permeating his body, but also as infecting everything he possesses or touches: no doubt by analogy with his odour; for a man's odour is a personal quality, distinguishable by dogs and (I believe) by some savages and hypnotic subjects; and the stench of his putrefying corpse may be believed to convey his courage and skill to those who inhale it[2]. And the savour of a burnt-offering is food for gods.

Savage ideas are generally so little thought out, and are so irregularly thought out by different tribes, that the relation of a thing to its soul-stuff varies widely from one tribe to another. In many cases the extraction of the soul-stuff from an offering by ghost or god may affect it so little, that the devotee or the priest proceeds to feast upon it; and I have nowhere met with the notion (which logic requires) that such metaphysically eviscerated food can only nourish a man's body and not his soul. However, since the eating of the sacrifice may be an act of communion with the ghost, he then naturally extracts the goodness only from his own share. In other cases, the breaking of weapons and utensils buried with a corpse implies an intimate unity between the wholeness of an object and its soul-stuff; and the Rev. J. H. Weeks says of the considerable wealth put into a grave by the Bakongo, that only the shell or semblance of anything is supposed to remain there[3].

This conception of soul-stuff may have been an important contribution to metaphysics. The doctrine of material substance is reached by abstracting all the qualities of things; but then there would be nothing left, were it not for this venerable idea of something invisible and intangible in things in which qualities may "inhere," or which may serve as a "support" to them. Along another line of speculation it may become the Soul of the World. When ghosts or spirits are no longer conceived to have bodies, a spiritual substance must be conceived for their qualities, in order to put them upon an equal footing of reality with corporeal things; but as there is no spirit-stuff ready made by the wisdom of our forefathers, this concept remains uncomfortably empty. In writing of Magic[4], I have indicated the origin of the notion of force; and if my view is justifiable, it appears that those celebrated abstractions 'force' and 'matter' may be traced to the savage mind. That savages are incapable of general and abstract ideas seems to be an illusion. It is true that the language of a people of low culture is very poor in abstract expressions; but no one now supposes that language is an adequate measure of thought. A language contains names only for things, groups, and aspects or actions of things, which the people who use it need to speak of: if they do not need to speak of abstractions, they have no words for them. But we cannot assume of the contents of the mind, any more than of the outside world, that things do not exist unless we have noticed and named them. Professor Franz Boas has shown[1] that languages that do not idiomatically express abstract ideas may be made to do so without violence, and that the abstract expression is intelligible to men native to the languages. "Everyone who knows peoples of low culture," says Dr Rivers, "must recognise the difficulty which besets the study of any abstract question, not so much because the savage does not possess abstract ideas as that he has no words of his own to express them[2]." When we are told that a certain tribe has a name for each kind of tree, but none for 'tree' in general, is it reasonable to infer that they have never received any impression of the resemblance of trees one to another, and of their difference from what we call herbs and grasses? Is there so little economy in the organization of the mind? When a tribe bases its grammatical gender on the distinction of Animate and Inanimate, has it in no sense corresponding ideas? An abstract idea results from a long process of dissociative growth from its concrete sources, and must exist in some manner at all stages of that growth, before its dissociation is completed by an appropriate name; and it is reasonable to suppose that at every stage of growth it functions and influences the course of thought. Accordingly, it is plain that the idea of causation exists obscurely in the savage mind[3]; so does the axiom "things equal to the same are equal" whenever a common measure is used, the foot for length, the fingers for number; and so do the notions of force and soul-stuff or 'substance.'

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[1] Hose and McDougall, *Pagan Tribes of Borneo*, II. 3.

[2] J. G. Frazer, *Belief in Immortality*, 403.

[3] *The Primitive Bakongo*, 371.

[4] This *Journal*, VII. 2.

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[1] *The Mind of Primitive Man*, ch. v.

[2] *Sociol. Rev.*, Jan. 1910, 9.

[3] This *Journal*, VII. 176.

VI. *Ghosts and Spirits.*

Whilst of some tribes, for example the Indians of Guiana, it is said that there is nothing to indicate that they "know of any spirits, except such as are, or once were, situated in material bodies[1]," amongst others we are often told that not only ghosts are known but also spirits that are declared never to have been incarnate. An extreme form of the ghost-theory maintains that all spirits were once ghosts whose incarnation has been forgotten; but this is needless, and seems not to be true. It is enough that probably the original inhabitants of the spirit-world were ghosts; that some of those now believed not to have been ghosts were really so; and that those spirits that were never ghosts are later immigrants, who have obtained domicile by having been imagined in analogy with ghosts. The following list indicates more or less probable reasons why (*A*) ghosts have sometimes come to be regarded as non-human spirits, and (*B*) why certain non-human things have come to be regarded as spirits of a similar kind.

A. Spirits that were formerly ghosts, but are now declared not to have been:

(*a*) Ghosts whose former life has been forgotten by mere lapse of time. The memory of the dead amongst many tribes does not extend beyond three or four generations. If then the ghost of some unusually impressive personality happens to be remembered, when all his relatives and contemporaries have been forgotten, he seems to be separated from the human race. And if, according to Spencer's hypothesis, his name had been that of some natural object, his ghost might now be regarded as the spirit of that phenomenon. But as to Spencer's hypothesis[2], although it gives such a plausible explanation of much nature-worship by real facts as to the working of savage language and thought that it seems to me unreasonable to doubt that it has had some of the effects he traces to it, yet it presses upon me more and more that most cases of nature-worship are to be explained by more particular causes.

(*b*) To dissociate a ghost from mankind is especially easy if his tomb has been forgotten, or if he has no tomb. As the drowned have no tombs, they easily become water-demons. Tombs must often be forgotten in consequence of migrations. In Central Melanesia both ghosts and spirits are recognised; but in the west worship is directed chiefly to ghosts, in the east chiefly to spirits. As migration has been from west to east, the tombs of ancestors can no longer be pointed out by the eastern islanders, and so their ghosts may have become spirits. In Tumloo (New Guinea) there are temples of spirits (all female) distinct from ancestral ghosts, and on the banisters of ladders leading up to these temples there are ornamental figures of ape-like animals; the architecture of the temples points to a former superior culture[1]. As there are no apes in New Guinea, these figures and temples may indicate a former residence under better conditions in Java or Borneo; and the spirits with which they are associated may be ancestral ghosts whose tombs and other earthly vestiges have been forgotten in the migration.

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[1] E. im Thurn. *op. cit.* 363.

[2] *Principles of Sociology*, §§ 165–193.

(*c*) We may see another way in which a ghost may become a pure spirit, if we suppose that as a ghost he had attained to some measure of worship, but that with the rise of new gods (by conquest, or by the reputation of being more helpful), or by his being himself too good to be worth worshipping, his rites have been neglected and his legend forgotten. Then he is no longer remembered as a ghost, or ancestor.

(*d*) It may be thought honourable to a god to deny that he was ever a man.

(*e*) The construction of a world-myth makes it necessary to begin somewhere with someone; and whoever becomes the first being, it is necessary to deny that he was ever begotten. But there may be inconsistent stories: the supreme being of the central Esquimo is a woman, Sedna, who created all things that have life; but other traditions give her a human origin[2].

B. Spirits that were never incarnate, but have been imagined by analogy with ghosts already propitiated:

(*f*) A Totem-ancestor may become a spirit; whilst, having himself no antecedents, he can hardly be a ghost as of an ordinary mortal. Nothing can be more irregular than the life of Totemism: with some tribes it seems to die out, or leaves few and doubtful vestiges; with others it continues to flourish even amidst conditions of high culture. Apparently, where it survives, the Totem-ancestor gradually loses his bestial properties, or most of them, and becomes an anthropomorphic spirit with his myths, in analogy with heroic or patriarchal ancestors. He has attained to a considerable degree of individuality; yet may, by association and tradition, confer more or less sacredness upon his animal kindred.

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[1] J. G. Frazer, *Belief in Immortality*, 220.

[2] Franz Boas, *Am. Bureau of Ethn.* VI. 1884–5, 583.

(*g*) Corn, rice, padi, nkàsa, or whatever has been the object of tribal rites and spells—the sun and moon, the earth, fire, wind and rain—having perhaps long been influenced and reinforced by Magic—become, when Animism has gained control of man's imagination, the embodiment or possession of spirits; the spells become prayers, and the rites religious ceremonies or mysteries. Such spirits, at first locally honoured, may with the evolution of the tribe or nation, the increasing intercourse of its villages, and the centralisation of its culture in some city, be released from local conditions and generalised into transcendent gods, either each of its own kind—corn or wine—or of still wider sway over agriculture or the weather. Plants from which intoxicants are obtained, which bring men to a condition resembling insanity, or the ravings of a sorcerer who is supposed to be possessed, are especially easy to understand as displaying the power of spirits. A belief in vegetation spirits, having originated in any way, may be extended according to the circumstances and mentality of a tribe, until every wood is populous with dryads.

(*h*) Natural objects that have, at first, been regarded with awe and therefore endowed with magical powers—mountain-tops, ravines, whirlpools—under Animism, become the abodes of spirits; and these, again, may, by analogy with others, cease to be conceived as merely local. Among the Moors, "the *jnūn*, which form a special race of beings created before Adam, are generally supposed to be active on occasions or in places which give rise to superstitious fear, and in many cases they are personifications of some mysterious qualities in persons or lifeless objects[1]."

In each of these cases, (*f*), (*g*), (*h*), however, an Euhemerist explanation may be offered. As to (*f*), Spencer, of course, argued that the Totem-ancestor is always a man, who bore the name of an animal, and was confounded with it after death. As to (*g*), Grant Allen suggested that the spirit of the corn or vine is always at first the spirit of the man upon whose grave the plant grew[2]. And as to (*h*), the spirit of a mountain may be the ghost of a man who was buried on the top of it; and the spirit of a whirlpool the ghost of a man who was drowned in it. Indeed some spirits may have originated in one of these ways, others in another way; and what happened in any particular case can only be determined, if at all, by examination of its particular circumstances.

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[1] E. Westermarck, *Marriage Ceremonies in Morocco*, 343.

[2] See his ingenious speculations in *The Evolution of the Idea of God*, ch. xiii.

(*i*) Abstract ideas may, at a very early stage of culture, be personified and treated as spirits. The Semang, according to Messrs Skeat and Blagden, personify Death, Hunger, Disease[1]; and the Beloki, according to the Rev. J. H. Weeks, attribute all personal qualities to the aid of spirits; so that if one man wrestles better than another, it is because the spirit Embanda is in him[2]. At a higher stage of culture we find Fides, Fortuna, Concordia; and the modern Greeks of Macedonia personify and propitiate Lady Small Pox[3]. Such things are conceived of as mysterious powers, and they have names; and so far they resemble demons. Why, then, should they not be personified and propitiated like demons?

(*j*) Various ways have been pointed out in which the grammatical structures of language may influence the growth of mythology.

(*k*) Animism having been generally adopted, spirits may be freely invented in explanatory myths. The Kalinis believe that thunder and lightning are the clang and flash of bracelets on the arms of Kidilumai, a girl who dances in heaven, as formerly on earth, for joy of the welcome rain[4]. It would be absurd to suppose that she must once have lived on earth. Some amongst the Ekoi say that Thunder is a giant marching across the sky; others that Thunder is the enemy of Lightning and, on seeing it, growls to drive it away[5]. If free invention may originate myths, it may modify old ones, with results that cannot always be interpreted upon general principles.

Finally, any spirits that have been anthropomorphised in analogy with ancestor-ghosts may be further disguised by giving them mythical family connections with the ancestors and with one another.

VII. *How Ghosts and Spirits are imagined.*

Ghosts and spirits have the same qualities and characters, eat the same food, appear in dreams, possess men and animals, help sorcerers, give diseases, determine the success of hunting or agriculture. At first, they are solid things, not properly incorporeal, merely invisible to ordinary people by daylight; though dogs or pigs may see them even then. A ghost is so associated with its corpse, that it is not always clear which it is that escapes from the grave and walks; and one may judge whether a dead man has yet gone to Hades by observing whether in the morning there are footprints around his grave; and to keep the ghost from walking, one may fill the belly of the corpse with stones, or break its limbs, or bury it deep and heap stones upon it. In South-East Australia, ghosts can be heard at night jumping down from the trees or from the sky[1]. They may be heard to speak or sing, usually with thin voices like bats:

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[1] *Pagan Races of the Malay Peninsula*, II. iii, c. 6.

[2] *Among Congo Cannibals*, 272.

[3] G. F. Abbott, *Macedonian Folklore*, 236.

[4] T. C. Hodson, *The Naga Tribes of Manipur*, 126.

[5] P. A. Talbot, *In the Shadow of the Bush*, 73.

> The sheeted dead
> Did squeak and gibber in the streets of Rome.

Spirits may have all the appurtenances of an animal body; for two of them waylaid an Australian, and made a wizard of him by taking out his entrails and filling up the cavity with the entrails of one of themselves[2]. They may marry mortals, as a devil begat Caliban upon a witch. In short, a ghost or spirit can act physically, just as a man can, because he has the same organs; but with greater power, because mysterious and more feared. And such beliefs persist amongst people whose culture is much higher than the Australian, as in Jacob's wrestling with something at the ford Jabbok, and Grettir's slaying of the ghost of Glam at Thorhallstad[3]. To this day, in Macedonia, there are vampires, or animated corpses, that throttle people and suck their blood[4]. "The Moslem corpse," says Sir R. Burton, "is partly sentient in the tomb[5]." The Karok of California consider it the highest crime to utter the name of the dead; for it makes the mouldering skeleton turn in its grave and groan[6]. In fact, it is difficult to think of one's own future corpse as entirely inanimate. According to Prof. W. Wundt, the Körperseele, as eine Eigenschaft des lebenden Körpers, is a starting-point of Animism independent of, and probably prior to, the breath and the dream, which suggest the idea of a free separable soul[7]. The confusion of ideas in popular Animism seems to me due to (1) the strong association of the ghost with the corpse, and the performance of rites (which must take place somewhere, if at all) naturally at the grave or in connection with relics; (2) the manifestation of ghosts as visible, speaking, tangible bodies in dreams; (3) the difficulty of imagining spirits to live and act except in the likeness of the body (though non-human forms—usually animal—are sometimes substituted); (4) the convenience of such imaginations to the story-teller; (5) the convenience of them to sorcerers and purveyors of mysteries, who rely upon such imaginations in producing illusion by suggestion. Accordingly, it is only in a late age, by metaphysical refinements upon 'mind' and 'matter,' or by mystical aversion to sensuosity, that the notion of incorporeal spirit is freed from these primitive qualifications, partially and amongst a few people. A tendency to abstract conception of the spirit is set up, indeed, in the ordinary way of 'dissociation' by the belief in transmigration. For if a spirit may 'possess' all sorts of bodies—men, plants, animals etc.—it is independent of any particular body; though it may still be thought to need *some* body. Where the idea of pure spirit has been established amongst educated people, it becomes necessary for those who believe in ghosts to explain how a spirit can manifest itself to eye, ear, nose, hand, without a physical body, by 'materialising' itself, as invisible vapour (say one's own breath in a frosty morning) condenses into a cloud or into dew; for the power of analogy is not yet exhausted.

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[1] Howitt, *Native Tribes of South-East Australia*, 437.

[2] Spencer and Gillen, *Northern Tribes of Central Australia*, 483.

[3] *Grettir Saga*. c. 35.

[4] G. F. Abbott, *Macedonian Folklore*, 217.

[5] *First Footsteps in East Africa*, 52 note.

[6] *Am Bureau of Ethn.* I. 200.

[7] *Mythus und Religion*, 2° ed. 78.

The varieties of belief that occur here and there in the world cannot be explained without a much fuller knowledge of local circumstances than is usually available. The Semang say that souls are red, like blood; no bigger than a grain of maize[1]; the Malays that they are vapoury, shadowy, filmy essences, about as big as one's thumb[2]; in both cases shaped like the owner. Elsewhere in the Indian Archipelago, "the animating principle is conceived of, not as a tiny being confined to a single part of the body, but as a sort of fluid or ether diffused through every part[3]." Amongst the Ekoi, the soul is a small thing dwelling in the breast, whilst a man lives; but at death expands into the body's full stature[4]. It may be the difficulty of finding the soul in the body that leads some thinkers to suppose it must be very small, others very attenuated—thin as a shadow and as breath invisible.

Whilst many savages believe, like ourselves, that the body entertains one soul and gives up one ghost, the Ekoi believe in two, one animating a man's body, the other possessing, or changing into, some animal in

[1] *Pagan Races of the Malay Peninsula*, I. 194.

[2] *Malay Magic*, 47.

[3] *Spirits of the Corn and the Wild*, I. 183.

[4] *In the Shadow of the Bush*, 230.

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the bush. Three souls, the vegetative, sensitive and rational, are well known to European philosophy. The Mandans thought that a man has one black, one brown, and one light-coloured soul; but that only the last returned to the Lord of Life[1]. The observer who tells us this also reports (p. 484) that some of the Dakotas assign to each man four spirits: one that dies with the body; one that remains with, or near, it; one that accounts for its deeds and at death goes to the spirit-world; and one that lingers with the small bundle of the deceased's hair, which is kept by relatives until they can throw it into an enemy's country to become a roving, hostile demon. In West Africa, too, Miss Kingsley found four souls: one that survives the body; one that lives with some animal in the bush; one, the body's shadow, that lies down every night in the shadow of the great god, and there recovers its strength; and, finally, the dream-soul. Some natives hold that the three last are functions of the first or true soul; but the witch doctor treats all four separately[2]. The shadow of a man, his reflection, his name, his totem, his breath, his dream-wraith, his corpse, supply natural starting-points for such speculations. Some Chinese philosophers held that "each of the five viscera has its own separate male soul[3]." I have found no belief in six souls; but Mr Skeat reports that, probably, in the old Animism of the Malays, each man had seven souls; though now they talk of only one; except in using spells, when the souls are addressed separately[4]. In the religion of Osiris there seems to have been a still greater number of souls: as of the name, the shape, the strength, the shadow, etc.: all reuniting with the immortal counterpart of a man's mummy, if justified at the last judgment. Professor Wiedemann suggests that these beliefs may have been collected from different local sources, and preserved for fear of losing anything that might be true[5]. Whereas, then, the prescientific mind is often accused of confusing things that are separate, we see here the opposite tendency to reify abstract aspects, and to separate things that are in nature united; and one probable cause of this is the practical interest of treating, and therefore of attending to and addressing, separately, certain aspects of a man in rites of exorcism, lustration, summoning, reinforcing, propitiation.

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[1] J. O. Dorsey, "Siouan Cults" in the *Am. Bureau of Ethn.* 1889–90, xi. 512.

[2] *West African Studies*, 200.

[3] Quoted by Sir J. G. Frazer's *Balder the Beautiful*, ii. 196–208, with other beliefs in a plurality of souls; in one case thirty, in another thirty-six.

[4] *Malay Magic*, 48

[5] *Rel. of the Anc. Egyptians*, ch. ix.

The belief in an external soul that exists apart from oneself (though identified with oneself for good or evil) in an animal, in the bush, or where not, may have arisen from the connecting of the soul with the shadow or reflection. The shadow is, indeed, attached to a man by the feet (except when he leaps), if the whole is seen; but it goes away at night: the reflection is quite separate, and is seen *within* a pool (as in a mirror), not on the surface, approaching us when we advance, and withdrawing when we retire: whence it is easy to understand that to take a man's photograph may be (with advanced animists) to take away his soul; and this may be a third way, besides two already mentioned[1], in which the substitution of a likeness for a person comes to be practised in Magic[2]. If this kind of soul may be some feet distant, why may it not be much further off, if there be any motive (such as the desire of secrecy) to wish or think it so? If it may reside within a pool, why not within anything else? What, in fact, becomes of it when we turn away? That it should be in an animal in the bush is reasonable enough, if one's Totem (even though imperfectly remembered as such) is an animal in the bush, and if oneself is in some sort that animal.

Beliefs as to the origin of souls sometimes bear the character of fanciful explanation myths. The Semang say that souls grow upon a soul-tree in the world of Kari (their chief god); whence they are brought by birds, which are killed and eaten by an expectant mother: souls of fishes and animals are also obtained by the mothers' eating certain fungi and grasses[3]. Here the analogy of the growth of fruits is adopted: being so familiar as to need no further explanation. Leibnitz's suggestion that monads are *fulgurations continuelles de la Divinité*[4], is at about the same level of thought. In other cases, we see the struggling to birth of ideas that still seem plausible: such is the widespread tenet that every present human soul is the reincarnation of an ancestor. We find this in Australia, in Borneo, in Manipur, on the Congo, in North America and elsewhere; and it has been adopted with a moral significance into higher religions. The Bakongo seem to base their belief partly on personal resemblance; upon which ground a child may be thought to have the soul even of a living man; so that to point out such a resemblance is displeasing, since it implies that, the child having his soul, he must soon die[1]. . Another reason they give for their belief is that the child speaks early of things its mother has not taught it, and that this must be due to an old soul talking in a new body. But Bakongo albinoes are incarnations of water-spirits, and greatly feared. Plato thought that, by a sort of law of psychic conservation, there must always be the same number of souls in the world[2]. Possibly in some cases people began by naming children after their ancestors, and later inferred that those who bore the same name must be the same persons.

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[1] This *Journal*, VI. 315, and VII. 185.

[2] The Malays, in their Black Magic, having prepared a wax image of someone who is to be injured, endeavour, before operating, to entice their victim's soul into it: for them the image alone is not enough. (Skeat, *Malay Magic*, 48.)

[3] *Pagan Races of the Malay Peninsula*, I. 194.

[4] *Monadologie*, 47.

As to the destiny of souls there seems to exist amongst the tribes of men even greater variety of belief than as to their origin. They may pass through more than one stage of development: as in the western isles of Torres Straits one becomes at death, first a *mari*, and later a *markai* with a more definite status[3]; or as with the Veddas, one is at first called "the living one," and only a few days later becomes a *yaka*[4]. Often the dead will be reincarnated, but the interval between death and rebirth may be passed in an underworld, or in a city in the forest, or indefinitely in a land of ancestors. They may turn into plants; as among the Mafulu old people's ghosts become large funguses growing in the mountains[5]: but more frequently into animals; either (from seeming caprice) into such things as termites or wild pigs; or (because wings seem to suit the spirit) into a butterfly or bee; or into owls or bats that haunt the night; or into deer or bear-cats, because these are seen in the clearings near tombs; or into snakes, because these are seen to come out of tombs, and often come into huts as if returning to their homes. They may also become stars, or shooting stars, or mere naked demons, or white men.

Though the ghost survive the body; and it may be said (as by the Ekoi) that it cannot perish, and the reason may even be given (as in the Bismarck Archipelago), that it is of different nature from the body[6], it is by no means always immortal. It may die, as it were a natural death, by oblivion; or, the next world being just like this, ghosts may fight together and kill one another[7]; according to the Tongans a ghost may be killed with a club; amongst the Bakongo it may be destroyed by burning its corpse[1]. It has been thought that to suppress the ghost was the original motive of cremation; but the western Tasmanians cremated their dead, and can hardly have done so to be rid of such mild Animism as seems to have been entertained by the eastern tribes, who buried their dead or abandoned them[2]. However, they seem to have been rid of it; whereas, in general, ghosts survive cremation, because this process cannot put an end to dreams. According to the Egyptians, the ghost participated in every mutilation of the body, and perished with its dissolution.

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[1] *The Primitive Bakongo*, 115.
[2] *Rep.* 611 A.
[3] Haddon, *Cambridge Exp. to Torres Straits*, 355.
[4] Seligman, *The Veddas*, 133.
[5] R. W. Williamson, *The Mafulu*, 266.
[6] J G. Frazer, *Belief in Immortality*, 396
[7] C. G. Seligman, *The Melanesians*, 658.

Whilst the ghost's life endures, its dwelling may be in the earth or sky, sea or forest, or in the land of the setting sun, or in the land of ancestors whence the tribe remembers to have migrated. Before departing to that undiscovered country, it may haunt the grave or the old home, till burial, or till the flesh decays, or till the funeral feast, or till death has been avenged. There may be one place for all ghosts, or two, or more, according to their age, or rank, or qualities (as sociable or unsociable), or whether or not their noses were bored; or according to the manner of their death, by violence, or suicide, or sorcery. Their journey thither may be short or long, an unadorned migration or rich in details of adventure. They may begin the new life exactly as they finished this one, or the old may be rejuvenated. As to their manner of life there, oftenest it is a repetition of their earthly state, perhaps better, or even much better, perhaps worse. And this conception is historically of the utmost importance: for (1) it seems to give the greatest confidence in a hereafter. Hume ascribes what seemed to him the incredulity of men with regard to a future life "to the faint idea we form of our future condition, derived from its want of resemblance to the present life[3]." And (2) from this conception proceeds the development of ghostly polities: presided over, according to tribes that have no chiefs, by a headman, such as Damarulum, or by the greatest known hunt-leader, like Kande Yaka; under advancing political structures, by chieftains, amongst whom one may be paramount, and so become a king or lord of all. It is everywhere an edifice built by hope and fear, under the guidance of analogy, and sometimes decorated by caprice, if this find acceptance with the tribe.

[1] J. H. Weeks. *The Primitive Bakongo*, 224.

[2] Ling Roth, *The Aborigines of Tasmania*, 57-61.

[3] *Treatise*, B. I., Part III, § 9.

VIII. *The Treatment of Ghosts.*

The behaviour of men toward the ghosts of their dead is chiefly governed by fear. The human power that has left the corpse is now invisible; that power, rarely quite trustworthy whilst in the body, especially when unobserved, retains its desires, caprices and hatreds that were partially controlled by social influences. What controls them now that the man is exempt from observation? How shall one defend oneself against him, or procure his neutrality, or even (as sometimes in the flesh) his help? The fear of ghosts has peculiar qualities: the invisibility of a spiritual enemy produces a general objectless suspicion and a sense of helplessness; associations with the physical conditions of the corpse and with darkness excite feelings very much like those aroused by snakes and reptiles. This fear may make the survivors quit the neighbourhood of the dead; sometimes makes them adopt means to induce the ghost to leave, and invent stories of how and whither he goes, which are believed by biological necessity; because, unless they can be rid of the ghost and the dread of him, or establish in some way the *pax deorum,* it is impossible to go on living.

The Yerkla-mining never bury their dead, nor in any way dispose of them. On seeing death approaching a tribesman, they make up a good fire for him, and leave the neighbourhood, not to return for a considerable time[1]. The Sakai, having buried the dead affectionately with necklaces, wallets, etc., say to him: "Do not remember any more your father, mother, or relations. Think only of your ancestors gone to another place. Your living friends will find food." They then burn his house and desert the settlement, even abandoning standing crops[2]. Among the Kikuyu, "if a person dies in a village, that village is often burnt, and the people trek off and build elsewhere," though much labour may have been spent on the surrounding fields. Sick people are often deserted[3]. Where land is closely settled such flight becomes impossible, and in any case it is very inconvenient; so that if anyone can believe it possible to frighten away the ghost by shouting at him, beating the air with boughs or firebrands, letting off arrows or guns, or to restrain him from walking by breaking the corpse's legs, or by placing loaded 'ghost-shooters' (straws filled with gunpowder) around the grave, so much the better. Many such plans are adopted, and they *must* be believed in.

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[1] Howitt, *Native Tribes of South-East Australia,* 450.

[2] Skeat and Blagden, *Pagan Races of the Malay Peninsula,* II. 95–9.

[3] Stigand, *The Land of Zinj,* 250.

Affection, however, has its part in the treatment of the dead: it is reasonable to attribute to affection the beginnings of the practices of leaving food at the tomb, burying weapons or ornaments with the corpse, celebrating funerary rites with lamentations; though in time this motive may be mixed with, or superseded by, fear of the ghost, or by fear of being suspected of having murdered the deceased by sorcery, should rites be neglected or maimed. It is not uncommon to carry about some bones of the departed, to hang round one's neck the skulls of infants untimely dead. The wild Veddas, though, having covered a corpse with boughs, they avoid the place for a long time for fear of being stoned, nevertheless have a strong feeling of good fellowship for the spirits of their dead[1]. The old Norsemen believed that the dead were still united with the living by intense sympathy[2]. In the eastern isles of Torres Straits, the Miriam perform an eschatological mystery, in which the recently deceased reappear on their way to the other world. The women and children take it for reality: their affections are said to be gratified; and at the same time their fears are allayed by the conviction that the ghosts, having been seen on their way to Hades, will no longer haunt them[3]. As rites begun in affection may become propitiatory through fear, and be further extended to obtain the aid of spirits in hunting, war, revenge, love, agriculture, trade, or any undertaking for subsistence, riches or power, every passion in turn seeks its gratification through Animism.

Extravagance in funerary rites may be checked by considerations of economy or convenience. Thus some tribes of South Australia may burn all the property of the deceased, but sometimes all except their stone axes, which are too valuable to be lost to the survivors. The Nagas bury with the body the things most closely associated with the dead; but things of small value, never the gun or the cornelian necklace[4]. The Todas, who burn their dead, lay the body on a bier with many valuable offerings and swing it three times over the fire; they then remove the money and the more valuable ornaments and burn the rest with the corpse. They say that the dead still have the use of everything that was swung over the fire; and tell a story to explain the ceremony; but Dr Rivers observes that "this symbolic burning has the great advantage that the objects of value are not consumed, and are available for use another time[1]." Economy may also induce the belief that ghosts are easily deceived, or are unaccountably stupid in some special way: as in the widespread practice of carrying a corpse out of its house through a hole in the wall; trusting that, the hole having been immediately repaired, the ghost can never find his way back; so soon does he forget the familiar door. This is cheaper than to burn the house down. Whilst often the most painful or disgusting rites are endured for fear of ghosts, at other times they are assumed to be so dull that we are tempted so say: "Whatever is convenient is credible." Superstitious practices may be carried out with self-destructive infatuation, or restricted at will: in Florida (Melanesia), in a certain stream, a very large eel was taken for a ghost; no one might bathe in, or drink at, the stream—"except at one pool, which for convenience was considered not to be sacred[2]." A conflicting desire creates a limiting belief.

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[1] C. G. Seligman, *The Veddas*. 131.

[2] Vigfusson and Powell, *Corpus Poeticum Boreale*, I. 417.

[3] *Camb. Exped. to Torres Straits*, VI. 253.

[4] Hodson, *The Naga Tribes of Manipur*, 100.

Imagination-beliefs, being swayed by moods and passions, are necessarily inconsistent. Natives of the Bismarck Archipelago are cannibals and greatly fear the ghosts of those they devour. Whilst feasting they hang up a slice for the ghost himself, and afterwards make an uproar to scare him away. Nevertheless they keep his skull and jawbone, which the ghost might be supposed especially to haunt; so easily do other passions overcome fear. Of the fear of ghosts sometimes seems true that which Bacon says of the fear of death, that there is no passion in the mind of man so weak but it mates and masters it. The Sia Indians of North Mexico had a masterful way of dealing with the ghost of a slain enemy: they annexed him together with his scalp; for this having been brought to the village, a shaman offered a long prayer, and thus addressed the ghost: "You are now no longer an enemy; your scalp is here; you will no more destroy my people[3]." The inconsistency (sometimes met with) of supposing a man's personal qualities to go with his ghost, and yet eating some part of his body to obtain those qualities, may be due to the latter practice having been magical, and having persisted after the rise of Animism. Some of the Eskimo have such control over ghosts by magic, that they fear them very little. After a death, the ghost remains peaceably in the house four days (if a man) or five (if a woman), and is then dismissed by a ceremony to the grave, to wait there until a child is born in the village, when it is recalled to be the child's tutelary spirit[1].

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[1] *The Todas*, 363.

[2] Frazer, *Belief in Immortality*, 379.

[3] Miss M C. Stevenson, "The Sia" in *Am. Bureau of Ethn.* 1889–90, XI. 121.

IX. *Evolution and Dissolution of Animism.*

Animism, originating in the belief in ghosts of men, tends to spread as the explanation of whatever had formerly been attributed to Magic (if we take this to have been earlier); although it is far from occupying the whole region that thus lies open to it. We have seen that the extent of its prevalence as an explanatory principle, and consequently as the basis of cults, differs greatly amongst different peoples. But more interesting than the spread of belief as to the agency of spirits so as to include more and more objects, is the gradual differentiation of some of them from common ghosts in power, character and rank, and their integration into families and polities. A process, going on for ages and varying with every people, cannot be briefly described: the work of Sir E. B. Tylor, Herbert Spencer, Prof. W. Wundt and others in this department is well known. In general it may be said that, allowing for the influences of geographical conditions and tradition and foreign intercourse, the chief cause of the evolution of a spirit-world is the political evolution of those who believe in it; so that the patriarchies, aristocracies, monarchies and despotisms of this world are reflected in heaven. Tribes of the lowest culture—some African Pygmies, Fuegians, Mafulu, Semangs, Veddas—have the least Animism; at successive grades—Australians, Melanesians, Congolese, Amerinds, Polynesians—Animism increases and grows more systematic; and it culminates in the barbaric civilizations of Egypt, Babylonia, Peru. But in civilizations of our modern type it rapidly loses ground.

The cause of such differences in the extension and elaboration of the animistic hypothesis cannot be that some tribes have had more time than others to think it out; since they have all had an equally long past. Animism is known to be very ancient, and there is no evidence that some races adopted it later than others. That, in the lower grades of culture, men want brains to think it out, is not a satisfactory explanation; because, on contact with superior races, backward peoples show themselves capable of much more than could have been inferred from their original state. Improvement in culture depends upon much besides native brains, namely, opportunities afforded by the resources of their habitat, and communication with other peoples.

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[1] Stefánsson, *My Life among the Eskimo*, 397.

The most backward peoples are the most isolated peoples. The development of Animism is entirely a matter of operating with ideas; and it seems to me that before men can build with ideas they must build with their hands; there must be occupations that educate, and give advantage to, constructive power, as in the making of boats and the building of houses and temples; for to this day a nation's material edifices are always, in clearness of plan, coherence and serviceableness, much in advance of their systems of theology and philosophy; because they must 'work,' as the Pragmatists say. Secondly, a suitable model for the edifice of ideas must be presented in the world of fact; and this, as we have seen, is supplied by the tribe's social and political structure; which, again, is clearly correlated with the improvement of architecture in building the houses of chiefs and gods. Thirdly, a favourable condition of the working out of any theory is to have the means of recording, either in oral literature or (still better) in writing, the advances already made; and, fourthly—a condition historically involved in the foregoing—the growth of a class of men, generally a priestly caste or order, sometimes poets, who have time to think, and the education and vocation to bring the accumulating masses of animistic—now religious—ideas to greater order and consistency. Hence there are two stages in the development of Animism—of course, not sharply separated: first, a long period of irregular growth in the tribal mind; and, secondly, a much shorter period, in which the extension of animistic theory depends more or less upon quasi-philosophical reflection.

With the differentiation of superior beings—heroic, ancestral or other gods—from common ghosts, Religion arises. As to the meaning of the word 'religion,' indeed, there is no agreement: some lay stress upon the importance of beliefs concerning supernatural beings; others upon prayer, sacrifice and other rites of worship; still others upon the emotions of awe and mystery. To define 'religion' (as a tribal institution) by all three of these characters seems to me the most convenient plan, and the most agreeable to common usage. That, on the one hand, pure Buddhism (if it ever was a living faith outside of a coterie of philosophers and saints), and, on the other hand, the spiritual condition of a few savages, may be wanting in one or other mark, is no serious objection. The former case is, in fact, exceptional and aberrant, and the latter rudimentary; not to give them the name of 'religion' in a technical sense is no wrong to anybody. Thus understood, then, Religion brings to the development and support of Animism many social utilities and other influences; especially the influence of dynasties supposed to have descended from the gods; and of priesthoods, whose sustentation and authority depend upon the supposed necessity of their intervention in worship. In their hands the free popular development of animistic ideas comes to an end, and gives place to the co-ordination of ideas by reflection, and to the dictation of tenets and rites by policy. The simple motives of hope and fear that actuated popular Animism are now supplemented by dynastic and priestly interests and ambitions; beneath which lies, faintly recognised and ill served, the interest of society in order.

This later sophisticated Animism, so far as it obtains a hold upon the people, is imposed upon them by suggestion, authority and deception. The part of deception in the history of Animism cannot be overlooked: it begins at the beginning, preceded, perhaps, and prepared for by the devices of Magic-mongers. Amongst the Arunta, women and children are taught that the noise of the bull-roarer during initiation ceremonies is the voice of the spirit Twanyirika[1]. Near Samoa Harbour, at harvest, they offer some of the first-fruits in a bowl to the ghosts; and, whilst the family feasts on the remainder, "the householder will surreptitiously stir the offerings in the bowl with his finger, and then show it to the others in proof that the souls of the dead have really partaken[2]." So early is the end supposed to justify the means. Animism, like Magic, strives to maintain its imaginations by further stimulating the imagination; and, in both cases, such practices are, with many men, compatible with firm belief on the part of the practitioner; who is merely anxious to promote the public good by confirming the weaker brethren: himself weak in the perception of incongruities.

Parallel with the development of Religion, a change takes place in the emotions connected with Animism. As the gods emerge from the shadow of night and the grave, and are cleansed from the savour of corruption, and withdraw to the summit of the world, they are no longer regarded with the shuddering fear that ghosts excite; as they acquire the rank of chiefs and kings, the sentiments of attachment, awe, duty, dependence, loyalty, proper to the service of such superiors, are directed to them; and since their power far exceeds that of kings, and implies the total dependence of man and nature upon their support and guidance, these sentiments—often amazingly strong toward earthly rulers—may toward the gods attain to the intensest heat of fanaticism.

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[1] Spencer and Gillen, *Northern Tribes of Central Australia*, 497

[2] Frazer, *Belief in Immortality* 259.

Extolled by priests and poets, the attributes of the gods are exaggerated, until difficulties occur to a reflective mind as to how any other powers can exist contrary to, or even apart from, them: so that philosophical problems arise as to the existence of evil and responsibility, and the doctors reason high

> Of providence, foreknowledge, will and fate,
> Fixed fate, free will, foreknowledge absolute,
> And find no end, in wandering mazes lost.

This is one cause of the dissolution of Animism: the power that comprehends all powers ceases to be an object, and becomes the immanence of all things, good and evil. Another cause is the complexity of theogonies, or of spiritual kingdoms with their orders and degrees: found too fanciful, when hierarchic despotisms, that furnished the analogues, give place to the simpler social structure of democracies; or, to a certain type of democrat, even insulting, a provocative disparagement of the sovereignty of the people. And Animism has other enemies in the growth of Positivism, and sometimes in the resurgence of Magic.

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THE THEORY OF REPRESSION IN ITS RELATION TO MEMORY[1].

BY ERNEST JONES.

THE following remarks represent in a sense a continuation of the discussion that took place at the Durham meeting last July when papers were read on this subject by Pear, Wolf, Mitchell, and Loveday[2], my object being to elucidate further certain points that were incompletely dealt with on that occasion and to raise certain other questions that are indirectly cognate to the same theme.

The theory itself may be formulated in several different ways, the most comprehensive of which would run somewhat as follows: There exist in the mind certain inhibiting forces, which tend to exclude from consciousness all mental processes the presence of which would evoke there, either directly or through association, a feeling of "unpleasantness" (*Unlust*). It is, of course, evident that the efficacy of such forces is at best a relative one, for otherwise consciousness would never experience *Unlust*, but the thesis is maintained that, whenever this experience occurs, it is only because the action of the forces in question has first been neutralised by other tendencies and motives in the mind, whether volitional or not. It will be noticed that in the definition given the word "exclude" has been used as a common term for what some writers consider to be two independent processes, namely, on the one hand the expulsion from consciousness of an "unpleasant" mental process, and on the other the tendency to prevent the return of this to consciousness on any subsequent occasion. As it seems possible to adopt one of these conceptions and not the other, they will be separately discussed, but I may remark that those who accept the theory of repression as here formulated are not much concerned to make a sharp distinction between the two, considering it to be relatively unimportant. For them the inhibiting force of repression is a tendency in constant operation, independently of whether the "unpleasant" mental process is of recent date or not, of whether it is at the time conscious or not, and even of whether it has ever been conscious or not, all matters which do not concern the main point at issue. The question of the relation of the inhibiting forces to consciousness and the unconscious is more obscure. Freud himself seems to place what he terms the "censorship"—an expression covering the sum total of the repressing forces in question—at the place of transition between the unconscious and the preconscious, with a less important one at the place of transition between the preconscious and consciousness. While it may be agreed that the action of repression is mainly exhibited at these points of junction, the evidence, in my opinion, induces one rather to picture the inhibiting tendencies as being distributed, in a streaming fashion, throughout the whole mind, conscious as well as unconscious, increasing in strength, however, as one proceeds from the level of consciousness to the lowest layers of the unconscious.

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[1] Read before the British Psychological Society, Jan. 30th, 1915.

[2] Published in this *Journal*, VII. No. 2.

The bearing that the theory of repression has on the problems of memory is a manifold one, and we may divide up the subject under the headings of registration, conservation, recollection, and recognition respectively.

I. REGISTRATION.

Practically all observers seem to agree that in attention there is a selective operation according to the pleasure-pain principle, whereby we attend to that which is pleasurable rather than to that which is not. (It is hardly necessary to say that no universality is claimed for this principle to the exclusion of others.) In so far, therefore, as variations in attention affect the registration of impressions, the process of exclusion, which is the essence of repression, must play a part. Its action, however, is by no means always in the direction that it might at first sight be imagined to be. It might be thought that, if repression determines the ignoring of some unpleasant sight or sound, these, being less attended to, would undergo a fainter registration and would fade more readily than other impressions. One should exercise caution, however, before coming to this conclusion, apparent as it may seem. To say that a given idea is unpleasant is not at all the same as saying it is without interest or significance to the subject, in fact the reverse is more often the case. Any psycho-analytical experience will provide numerous instances where an idea that has been presented to consciousness has been immediately ignored and its meaning not apprehended, but has nevertheless made a deep impression on the subject's mind, and has been registered with unusual distinctness. It is quite common, for example, for girls in the period of adolescence genuinely to ignore in an astounding manner all allusions to sex matters, which they meet with at every turn, in newspapers, in novels, and in daily life, and it is remarkable to observe later in a psycho-analysis the precision with which these impressions have been registered and their significance appreciated without the subject being at all consciously aware of it. It is also a commonplace observation in insanity that in a state of excitement or delirium certain expressions may be reproduced by a lady who certainly must have completely ignored them at the time of hearing them[1].

The only means of estimating the intensity of registration is by studying the conditions of subsequent recall, conditions which are themselves influenced by various other factors that are not easy to eliminate or allow for, but so far as the evidence goes it does not, in my opinion, favour the view that incoming impressions which are ignored through the action of repression are more faintly registered than others which have not been submitted to this action.

Much might be said concerning the selective action of repression in regard to attention (and also to perception) that is of importance to the general theory of attention, but as the subject is not strictly germane to the problems of memory it must be passed over here with merely this short allusion.

II. CONSERVATION.

The theory of repression, and the observations on which it is founded, has one very important contribution to make regarding the conservation or retention of memory-impressions. It is to the effect that the capacity of the mind for such retention is vastly greater than is generally supposed. It is difficult to lay too much emphasis on this point, one to which attention has repeatedly been called by medical psychologists, for even after a considerable experience of such data one is constantly being surprised at the truly extraordinary manner in which minute details may be revived after having apparently been completely forgotten for many years, for half a century or longer. This applies both to significant, repressed memories and to those of a more trivial nature. With regard to the latter class I must say that I become increasingly sceptical about its supposed involvement in what Mr Pear[1] calls a process of physiological decay, for delicate methods of investigation constantly succeed in demonstrating the continued presence in the mind of trivial elements that one might have imagined had disappeared long ago. At the same time, the distinction Mr Pear draws between the two classes of forgotten material, the significant and the insignificant, appears to me to be of great importance, and the explanation of the process of forgetting in the two cases raises some interesting questions, to which I shall return later in this paper.

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[1] A well-known literary example is the case of Ophelia.

III. RECOLLECTION.

Professor Brough has somewhere remarked that psychologists may be divided into two schools as regards the subject of memory, comprising those who hold that the facts in most need of explanation are those of remembering, and those who hold that they are the facts of forgetting. Psycho-analysts certainly belong to the latter school, which I imagine to be the less numerous one. To us, and contrary to the usual opinion, it would seem more natural if everything were remembered, and it is the circumstance of a given mental impression not being capable of recall, while still in the mind, that demands an explanation. A consideration to which I would attach great importance is that the phenomenon of recall seems to be too often regarded as being induced by conscious volition, the fact being overlooked that in the majority of instances it proceeds quite spontaneously and independently of volition. If, now, the matter be thought of in other than volitional terms, the question is, not what is the mechanism whereby we recall a given idea when we want to, but rather how is it that such a relatively small number of ideas actually flow into consciousness, or have the capacity of ever entering consciousness? To this question Freud's theory of repression attempts to provide an answer.

The main fact that this theory sets out to explain is that it is more difficult to recall an unpleasant memory than a pleasant one, other things being equal. The fact itself is, I think, beyond dispute, and has not been questioned by anyone who has seriously investigated the phenomena, either experimentally or clinically. According to the theory of repression, the reason why it is more difficult to recall an unpleasant memory is because it is kept back from entering consciousness by the action of certain inhibiting, "repressing" forces, the function of which is to guard consciousness, so far as possible, from the pain of disagreeable affects. Starting from this simple conception, the validity of which will presently be discussed, the theory has undergone two extensions. The first of these was made by Freud in his earliest monograph on normal psychology (1898)[1], and was to the effect that repression accounted for the difficulty in recall, not only of obviously unpleasant memories, but also of a large number that, so far as could be seen by direct introspection, were not of this nature. The class referred to comprises the unexpected failures in memory, the failures that cannot be accounted for by the usual factors; examples are the temporary forgetting of familiar names, of well-known pieces of knowledge, and the like[2]. The second extension of the theory, for which I am willing to bear the responsibility—although I believe there are other psycho-analysts who would also be prepared to go as far—concerns the application of it to all forgetting, maintains, in short, that all forgetting is due, in part at least, to repression. I shall presently adduce some considerations which may possibly make this last generalisation seem less preposterous than it appears to be at first sight.

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[1] *Loc. cit.* p. 139.

The questions raised by the theory correspond with the three stages of it just mentioned and may be formulated as follows: Is there such a process of repression which accounts for the difficulty in recall of unpleasant memories? If so, can it also apply in the case of memories that are not in themselves of an unpleasant nature, and, further, can all forgetting be attributed to it? These questions will now be discussed in this order.

A. Evidence of Repression.

The principal fact to be explained is the greater difficulty of reproducing a memory when this is invested with unpleasant feeling. That this is a true statement of the case may, I think, be assumed here; if necessary, an overwhelming amount of evidence could be adduced in confirmation of it. Some five different explanations have been proffered, and, although there is little to be said about some of them, they will all be mentioned for the sake of completeness.

1. We have first the usual explanation given both by psychologists and by the laity, namely, that the diminished capacity for reproduction

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[1] Reprinted in his *Zur Psychopathologie des Alltagslebens*, 4te Aufl., 1913

[2] Numerous instances are given in my *Papers on Psycho-Analysis*, 1913, ch iii.

of certain mental processes is due to some defect inherent in these processes. This explanation is put in various ways, the factor being described sometimes as a "lack of interest," sometimes as a "reduction in the investment of psychical energy." Jung seems to approximate to this position when he speaks of buried infantile complexes having lost their significance owing to the development of the individual on to a higher plane, so that he is no longer interested in such childish matters. The view that the difficulty in reproduction is to be accounted for by a lack of interest in the mental processes concerned can only be held by those unfamiliar with the findings of medical psychology (hypnosis, psycho-analysis, etc.), for in these it is a matter of daily experience to meet with great difficulty in resuscitating memories, invested with vivid feeling, that are of immense interest and significance to the personality. Jung himself gets round this argument, at least so far as infantile complexes are concerned, by asserting that, although these have become deprived of their importance in the course of development, they are later re-invested by feeling and significance of a different source. In my opinion this is a hypothesis in favour of which there is no good evidence, but in any case it concerns only a selected group of memories and does not touch the general principle that memories may be very difficult to resuscitate even when they are charged with intense significance to the personality. Lack of interest, therefore, cannot be the cause of the difficulty in reproduction.

2. An ingenious explanation has been put forward by Dr Wolf[1], which runs as follows: Accepting the conception of repression only in the limited sense of a volitional expulsion of unpleasant thoughts from consciousness, he suggests that the difficulty experienced in reviving repressed, unpleasant memories may be due to subsequent repression of any incoming ideas that by association would tend to recall them. This description, however, is not at all in accord with what actually happens when a forgotten unpleasant memory cannot be brought into consciousness. An associated idea that one would expect to evoke a given memory may fail to do so even while it is being held in the very focus of consciousness, there being no question of its being expelled from consciousness altogether. Then, again, the situation is very much the same when there is no conscious associated idea present to be repressed, such as in the search for unknown buried memories in the procedure known as "free association." Further, it is hardly possible on this hypothesis to account for the difficulty in the evoking

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[1] This *Journal, loc. cit.* p. 150.

of buried memories that never have been conscious. Everyone will agree with Dr Wolf in the stress he lays on the important part played by the associated, derivative ideas in the matter of evoking unpleasant forgotten thoughts, but it is not giving a true picture of this part to describe it as the entering of such ideas into consciousness and their subsequent expulsion from it *seriatim*, for the hindrance is further back—in the difficulty they experience in approaching consciousness.

3. Dr Mitchell[1] suggests that repression may be "a passive resistance, a mere hindrance or obstruction, felt as resistance but necessitating no activity on the part of whatever causes the obstruction." This suggestion was thrown out as a possible alternative to the psycho-analytical view, and certainly describes what often happens in the physical world. I do not know of any analogy to it in the mental world, however, and as no data or arguments are given in support of it—the whole suggestion being meant to be most tentative—I am not in a position to criticise it, though I might add that I cannot think of any consideration or evidence pointing to it.

4. Another alternative put forward by Dr Mitchell[2] is that unpleasant memories are not pushed or repressed out of consciousness, but, on the contrary, are drawn out. He writes: "It is just to some psychical force analogous to the pull of gravity in the physical world that the main work of repression is ascribed by psycho-analysts at the present time....It is the attraction of the unconscious rather than the repulsion of the conscious that leads to the forgetting and causes resistance." Dr Mitchell is in error in his first sentence here, for there is certainly no psycho-analyst who ascribes *any* forgetting directly to the attraction of the unconscious, and clearly to ascribe "the main work of repression" to such a process would be a contradiction in terms. In a rather obscure passage, Jung also has allowed himself to be guilty of this contradiction, saying "in these cases it seems as if the mechanism of repression were much more in the nature of a passive disappearance, or even as if the impressions were dragged beneath the surface by some force operating from below." If we ask what this force operating from below is, we are told it is the attraction of association, but those who have freed themselves from the associationist doctrine are unable to perceive here any dynamic force capable of being opposed to the efforts to recall a buried memory. Even the difficulty in bringing to consciousness the primary, "attracting" group of ideas Jung conceives not as denoting a conflict of forces, but as being due to a lack of sufficient

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[1] *Loc. cit.* p. 157.

[2] *Loc. cit.*

development on the part of these ideas or else to a loss of interest in them, an explanation discussed above. Passing by these objections, however, it is plain that this "tug downward" explanation could at best be merely a partial one, since it only relates to ideas that might be drawn downward by the attraction of previous ones, and not to the latter themselves. The existence of the "tug" has first to be demonstrated before we can discuss its possible field of operation, and this preliminary task has not yet been attempted.

5. The psycho-analytical explanation is precisely the opposite of the last one, being to the effect that the force maintaining forgetting acts not from the unconscious, but from the direction of consciousness. The evidence in favour of it is derived from manifold sources, of both an introspectual and observational nature.

In the first place, both historically and in point of importance, the behaviour of a subject undergoing psycho-analysis admits of no other explanation than the present one. In this procedure he is asked to adopt the method of "free association," the aim being to create a pathway into consciousness for the emotion attached to buried complexes, which in the case of a neurotic patient lie at the basis of his symptoms. He has to relate all the thoughts that come into his mind, irrespective of what his opinion may be of them. If this is done, the directing of the inflowing thoughts, freed from the guidance of either conscious reflection or external stimulation, is taken over by the emotional complexes of personal significance. These are approached only gradually, first by way of more or less distant allusions to the underlying theme. Now, when this happens, *i.e.* when the painful mental processes are being approached, the subject's behaviour undergoes an interesting change. He breaks his simple instructions and omits to mention all the incoming thoughts, making a selection of them on various grounds, he consciously guides his thoughts into some other more agreeable direction, *i.e.* changes the subject, he resents having his attention called to what he is doing or being reminded of the rule he has to follow, and he may even attempt to enter into an argument or dispute with the analyst. Simultaneously his external conduct undergoes a corresponding change. He invents all manner of plausible excuses for being late at the next appointment, or misses it altogether. When he comes he is either prepared with a flood of material that precludes any return to the subject previously touched on, or he is barren in the production of thoughts, which remain quite superficial. In short, there can be no doubt whatever that a spirit of opposition has been aroused, which actively strives against the evocation of the unpleasant buried emotion. This opposition is undeniably a dynamic force, and to it is given the name of "resistance," which is merely the obverse of repression. It means that something in the subject is fighting against the reproduction of whatever unpleasant emotional complexes may lie in his unconscious, and the simple conclusion drawn from these phenomena is that inhibiting, repressing forces are striving to prevent the complexes from entering consciousness, a conclusion which is the substance of the theory of repression. The same sense of resistance just referred to can be detected, though not so readily, by anyone who is carrying out a psycho-analysis on himself.

This conclusion is confirmed by our definite knowledge that inhibiting forces of the kind in question do exist in the mind, though their existence is mainly admitted in another connection. Namely, we are all aware in ourselves of an instinctive tendency to "turn our mind away" from certain thoughts of a personal nature, to avoid and ignore certain classes of impressions from without and certain thoughts that rise into consciousness from within[1]. It is also easy to perceive evidence of the same tendency in those about us, particularly when the thoughts against which they strive are of a kind to which we ourselves are indifferent. For my own part, further, I think that introspective examination of one's attitude on these occasions shows the existence not only of a tendency voluntarily to expel certain thoughts out of consciousness, but also of a tendency to prevent them from entering consciousness. And it is to the constant pressure exerted by the latter tendency that psycho-analysts for the most part refer when they use the word "repression."

B. *Indirect Action of Repression.*

Some writers are disposed to admit the existence of repression in the full sense indicated above, but would limit its scope to occasional instances of obviously unpleasant thoughts which have been buried by repression. As was mentioned above, however, Freud has extended the conception of repression to cover instances of the burying and forgetting of thoughts that do not appear to have any unpleasant content, and where the desire to recall them may be very pronounced, such as in the "slips of memory" where there is a fruitless endeavour to recall some familiar piece of knowledge. A common objection made to this extension of the theory (for instance, by Professor Stout) is the question, "How is it possible that a failure to remember is due to a wish to forget, when one may very much want to recall the memory concerned?" It seems to be overlooked here that the same mind may simultaneously contain two exactly opposite impulses: for instance, the desire to recall something for one reason and an antipathy against recalling it for another, and that the subject may be entirely unaware of one of them, *e.g.* the latter. This matter must next be considered more in detail.

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[1] The universality of this tendency is apparently contradicted by the not infrequent occasions on which a person may be attracted, or even fascinated, by disagreeable thoughts. Apart, however, from the consideration that the very fascination proves the existence of pleasurable emotions that may have been strong enough to counteract the tendency to repress "unpleasant" ones, we have to remember that these thoughts are usually substitutes for still more unpleasant ones that repression has kept buried.

According to Freud, the action of repression extends over from the original object of the repression, the given unpleasant complex, to the other mental processes that may stand in an associative relation to this. The reason for this is as follows: The affect investing the painful complexes is in a state of high potential and so tends to radiate on to whatever ideas that become associated with them—the process known as "displacement of affect." In other words, the secondary ideas become implicated in the affective constellation of the original one, and therefore become subject to the same affective influences as these are. Now, among these influences is the inhibiting effect of repression; hence the action of repression affects the secondary groups as well as the primary ones, though of course in a less degree. The practical result of this is that thoughts may be kept from consciousness not on account of their own significance, but because of their relation to other thoughts that are in a state of repression on account of their "unpleasant" content. The occurrence is really of everyday familiarity: we instinctively avoid various thoughts because they might remind us of other thoughts that we do not wish to recall. Put in another way, the counter-force that prevents recall may be directed not immediately against the thought one is trying to recall, but against another thought, and the former may be involved only through the affective bond attaching it to the latter. To use a topical analogy, the associated ideas are non-combatants that get in the line of fire.

In his paper Mr Pear relates an instance where he could not recall a certain name until he noticed that it was nearly associated with the name of a dead friend in connection with whom there existed some painful emotion. I do not doubt the accuracy of his analysis, for I have had numerous experiences of a similar kind. Against his interpretation Dr Wolf[1] brings the objection that if the forgetting of the first name were due to repression of the second one then the latter should have been inaccessible to consciousness, *i.e.* also forgotten. This raises a very interesting point, and also gives the opportunity for calling attention to a matter of fundamental importance that is commonly overlooked in discussing the theory of repression. There seems, namely, to be a prevailing notion that repression is mainly a question of forgotten ideas, whereas the truth is that the whole problem is essentially one of the affective life. Both the repressing force itself and the mental material that is repressed can most accurately be described in affective terms, and the ideational content of the latter is only implicated in the memory process in so far as it is invested with the repressed affect. The so-called "painful idea" is really only a sign that represents the whole complex, and when the latter is in a state of repression the idea itself may or may not be inaccessible to consciousness, *i.e.* forgotten, according to circumstances. There is, indeed, a type of mental behaviour in which these ideas become divested of the affects belonging to them—the two being "dislocated" from each other—a process for which some writers would not scruple to coin such a word as "de-emotionalising." When this is very pronounced, the personality becomes of the coldly intellectual type, all mental processes being "rationalised" to an extreme degree and the conscious affective life reduced to a minimum. In the obsessional neurosis this occurrence is characteristic even of the pathogenic complexes themselves, so that, in contradistinction to hysteria, the patient may have introspectual access to the ideational content of these complexes, although the affective investment of these is completely repressed. In therapeutics this is a matter of great practical import, and that is why I described above the aim of a psycho-analytic investigation not as an endeavour to make buried ideas conscious—this being merely an indicating sign, so to speak, of the main procedure—but as the creation of a pathway into consciousness for the emotion attached to buried complexes.

Returning now to Mr Pear's case, one can see that it is probably an example of this process. What one supposes to have happened is that the affect connected with the dead friend was still partly repressed, and so associated ideas that might arouse it tended to be repressed also, *i.e.* prevented from entering consciousness. In such a case it might well happen that this keeping of ideas out of consciousness would apply more forcibly to secondary, associated, and less important ones than to the primary ideas themselves, which is the paradox that puzzled Dr Wolf. Yet it is a fact constantly to be observed clinically and is also capable of experimental confirmation; it lies at the basis, for instance, of Jung's diagnostic association test. It seems that an indirect association is more efficacious in arousing a repressed emotion than a direct one. The explanation of it is that the defensive mechanism which we called "dislocation" above is brought into action more extensively in the case of the primary ideas of the complex than it possibly can be with all the ramifications of associated ideas. This is because it is more important that it should be done in the former case, and, again, the association with a secondary idea may be instantaneously forged before there is time for this mechanism to come into operation. The mechanism, which is functionally (and also in its conscious origin) akin to the "secondary elaboration" affecting dreams, deals first and foremost with the most dangerous points, *i.e.* the primary ideas of the complex. We are all aware in daily life of how a direct reference to a painful event may leave us untouched, for we are more or less prepared for it and therefore fortified against it, while an unexpected allusion in an indirect way may arouse almost the full strength of the original emotion.

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[1] *Loc. cit.* p. 152.

C. All Forgetting due to Repression.

Coming now to the thesis that all forgetting is due in part to repression, in order to make it at all comprehensible how anyone can possibly hold such an extreme view I have to introduce two further considerations, of which one concerns an observation easily capable of confirmation, the other an hypothesis of a more speculative and theoretical nature.

The observation is this. In working with psycho-analysis one finds that the unconscious material in the mind is very much more extensive than might be surmised, that the assimilative capacity of the complexes, due to the radiation of affect, is very much greater, and that, therefore, the number of associations that are established in the unconscious is simply enormous. That being so, it is extremely difficult, and at present impossible, to set any limits to the extent to which operations characteristically applying to unconscious material, such as repression does, are in action. One is practically never in a position, for instance, to assert that such and such an idea cannot have been associated with any "unpleasant" buried complex, for to be so would necessitate a most searching investigation of all its associations, both conscious and unconscious. It is rather like the question of the alleged destruction or fading of forgotten memories, a negative proposition that it is impossible to prove. One can only say, with considerable emphasis, that the more extensive the investigation the greater is the number gotten ideas that prove to be affectively connected with repressed exes, so that the possibility is at least open that they all are.

e hypothesis alluded to concerns a widening of the connotation e concept "repression," and necessitates one or two preliminary arks. According to Freud[1], all mental activities can be summarised under two fundamental principles, which he terms the "pleasure-principle" and the "reality-principle" respectively. The first is the well-known principle of the search for pleasure and the avoidance of "pain" (*Unlust*), and is the more primitive of the two. The second may also be described in terms of "adaptation to life," "adjustment to reality[2]," and so on. In his essay on the subject Freud indicates the probability that the second principle is genetically derived from the first, a matter concerning which there is bound to be much difference f opinion. To those who accept this view, however, the following nsideration presents itself. The conception of repression, as developed Freud, is purely a hedonic one, the function of repression being lusively to avoid pain. Now it is a very common occurrence in mind that a tendency which has been developed to serve one function ts appropriated, so to speak, to serve the purpose of another one as well; many examples could be given of this. And it seems to me thoroughly plausible that this is what has happened in the case of repression. Leaving aside the whole question of "sublimation" with its relation to repression on the one hand and to the reality-principle on the other, about which much might be said in this connection, and confining ourselves to the present theme of forgetting, we cannot but be struck by the purposiveness of most of the ordinary acts of forgetting. To have one's consciousness burdened with all manner of irrelevant memories and other mental processes when one is concentrating one's attention on a limited problem of the moment would evidently be highly deleterious to one's efficiency in dealing with reality, and I think it is the generally received opinion among psychologists that this is the direction in which we have to seek for the further understanding of the ordinary problems of remembering and forgetting. With a mechanism of repression already established for the purpose of excluding from consciousness various disturbing mental processes—for it should not be forgotten that repression is in action from the earliest periods of life- it would seem only natural that it would be seized upon and employed for the more utilitarian function of excluding also irrelevant ones when the time comes for the application of the mind to the problems of reality. Indeed, the disturbance to consciousness caused by the intrusion of these irrelevant and useless thoughts might without much exaggeration be conceived of in terms of the pleasure-principle itself, as a mild variety of *Unlust*. Be that as it may, I suggest it is more than possible that ordinary forgetting is, at least in part, brought about by a tendency which is a development of repression in its original sense. For the sake of convenience one might perhaps speak of "hedonic repression" and "utilitarian repression" respectively.

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[1] Freud, "Formulierungen über die zwei Prinzipien des psychischen Geschehens," *Jahrbuch der Psychoanalyse*, Bd. III. S. 1.

[2] It is hardly necessary to say that the term "reality" does not here apply to the physical world alone.

A piece of confirmatory evidence of this view is to be obtained from the study of some neurotic symptoms. When under certain circumstances repression fails in its function of satisfactorily excluding various complexes from consciousness, certain results follow, notably two: o the one hand, disturbing manifestations of these complexes, clinica termed "symptoms," obtrude themselves on consciousness against [illegible] person's (conscious) will, while on the other the affective ene belonging to them can no longer be transformed ("sublimated") a devoted to the purposes of the reality-principle, *i.e.* to adjustmen and achievement in life. Now it is interesting to observe that precisely parallel results also follow in the sphere of what we have called utilitarian repression. On the one hand the person is disturbed by the obtrusion into consciousness of all sorts of irrelevant and distracting thoughts and memories, while on the other he is unable (more or less) to call up those that are necessary for the ordinary purposes of life. He presents, in other words, the common picture of a patient who cannot concentrate his mind on any subject, partly because he cannot think of the things he should, and partly because he has to think of things he shouldn't (for the purpose in hand). I will not develop this clinical aspect of the subject further here, although there are a number of other observations that, to my mind, speak in the same sense of an inherent inter-relationship between the two forms of forgetting (the ordinary and that due to hedonic repression), one that corresponds with the relationship between the reality-principle and the pleasure-principle.

IV. RECOGNITION.

The full purpose of recollecting is not fully achieved until the object of memory is not merely recalled to consciousness, but also collated with its intrinsic associations, and its proper meaning and significance appreciated. In this latter process there are many stages, and it may be possible to accomplish some of these but not others. In hysteria one can sometimes observe the most beautiful dissections of even the elementary attributes of recognition, such as sensation of a physical object, localisation, form, use, and name[1]. Dissection of the more complex attributes are, of course, of everyday occurrence, such as when one recalls certain connections of a given idea, but not others.

This matter of partial recognition is one that has regularly to be investigated during the course of a psycho-analysis, and the question has to be answered why certain connections only of the given idea have been remembered and appreciated, while others, often more important ones, have not. In such an investigation one constantly meets with examples in which the action of repression, through the associations
ith various buried complexes, can be clearly demonstrated as the
sative factor in bringing about this isolation of certain elements of
ntal constellation and not others. I would therefore maintain
n the study of recognition, as of memory altogether, the appre-
of the selective influence of repression is of cardinal importance.

published a study of a case of this very sort: "La vraie aphasie tactile."
logique, Jan. 1907, p. 1.

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THE RECOGNITION VOCABULARY OF CHILDREN.

BY GODFREY H. THOMSON AND FRANK W. SMITH.

THE experiments briefly described in the following note were carried out in the Spring of 1914 at Birchfield Road School, Liverpool[1]. They were only preliminary to further investigations; but, as the outbreak of war has hampered these, we think it advisable to publish them as they are. The method followed was similar to that employed by Kirkpatrick and described in Professor Whipple's *Manual of Menta and Physical Tests* (pp. 458–464). Some modifications were howe introduced. The essential point about Kirkpatrick's method is t a sample of words from a dictionary is submitted to the subjec [illegible] marks those which he understands, and is if necessary tested to s [illegible] his marking is free from error. If he knows say 50 per cent [illegible] words in the sample then he most probably knows half t [illegible] dictionary. The dictionary we employed was Chambers's [illegible] *Century Dictionary* which contains about 35,000 label-wo [illegible] samples were chosen purely by chance. by a system of card [illegible] In all 170 words were thus selected, and were presented to boys and girls of ages from nine to fourteen, from twelve classes in the school. At first only ten words were set in each test, but it was felt that a longer test would be more satisfactory and in the later experiments twenty words were set. The following is a typical test: Tsar, Protoplasm, Ensilage, Curve, Heathen, Peregrinate, Gorgeous, Habitable, Wick, Expedient, Scilla, Peotomy, Blore, Polt, Buphaga, Macrobiotic, Vivers, Rudmasday, Ovolo, Crenel. Words obviously known to all children—*and*, *but*, *wash*, etc.—were credited to all, words quite beyond their powers were debited to all. The remaining words of the test were put on the blackboard, and each child gave its impressions of the meaning of each word.

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[1] The actual work was entirely carried out by Mr Smith (who is headmaster of th school) and his staff. I am only responsible for suggestions. G.H.T.

y were t required to give a formal definition, but were expected a pass to show that it could be understood if met with in a passage English, or in the course of conversation. The teachers marked their l exercises. Any case which appeared doubtful from the written rcise was investigated orally. From time to time meetings of the chers concerned were held with the object of securing a uniform ndard of judging. In very few cases was a word on the border line d—it was either known or unknown. Owing to occasional it did not happen that every child tested tried every test. results average out as having been taken by 238 boys and

TABLE I.

Age	Boys			Girls		
	Aggregate words set*	Correct	Percentage	Aggregate words set*	Correct	Percentage
9½	3140	461	14·7	3160	461	14·6
10½	6800	1043	15·3	9800	1492	15·2
1½	10540	1644	15·6	7910	1305	16·5
2½	10100	1826	18·1	8270	1425	17·2
½	8870	1838	20·7	8300	1581	19·0
	990	204	20·6	1490	294	19·7

words were set. The aggregate number would be 170 times the number given age were it not that some children were occasionally absent.

are more interesting when given as curves showing the olute number of words known at each age, assuming that the tionary contains exactly 35,410 words[1]. (See Fig. 1.)

The results differ considerably from Kirkpatrick's tests (Whipple, 461): but it would be unprofitable to discuss this here, for more eriments are needed. The use of a different dictionary, and the jective standard adopted, are doubtless important factors. Probably curves are most unreliable near the ends. Not only are the numbers er at the extreme ages; there is also some selection, for at the top the school the clever children (especially the boys) tend to leave ly with scholarships for secondary schools. Even near the middle the curves the probable errors are no doubt considerable. Their er of magnitude can be gathered from the following considerations.

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[1] This is the number obtained in an actual count by school children taking a few pages 1.

(1) There is a sampling error due to the fact that only 170 words were tested out of the whole dictionary. The sample may have been unduly hard or unduly easy. The probable error due to this cause near the middle of the curve where about 16 % of the words are known is

$$\cdot 6745\sqrt{\frac{0\cdot 16 \times (1 - 0\cdot 16)}{170}} = 0\cdot 019,$$

that is about 1·9 %, or about 700 words.

Extent of Vocabulary estimated from a test of 170 *words.*

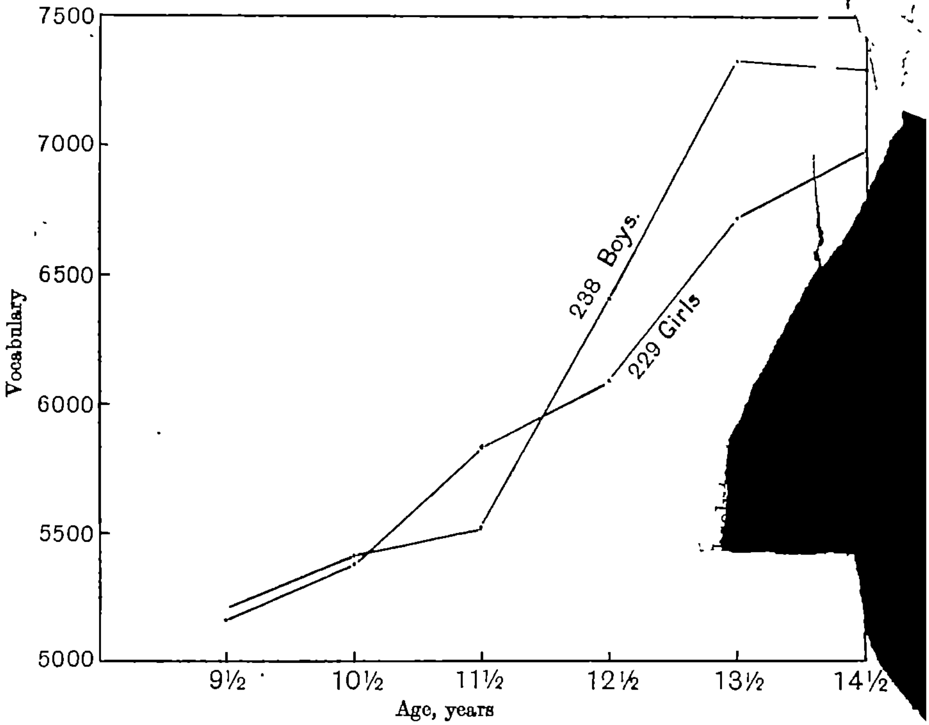

Fig. I.

(2) There is also a scattering of results among the different children. Unfortunately the records were not kept in such a way as to enable this to be measured exactly. Fig. 2, which is based on four tests of twenty words each, shows that in this number of words the semi-interquartile range is about one word, or 5 %. More accurately it can be calculated to be 3·56 % and the probable error of the mean is then 0·74 % or nearly 300 words. For boys or girls alone it will be greater,

say about 400 words. This probable error only has a meaning as such if we think of the children actually tested as a chance sample of a larger community. Moreover it must not be too hastily assumed that (1) and (2) are independent.

58 *eleven-year-old children of both sexes. Average of four tests of* 20 *words each.*

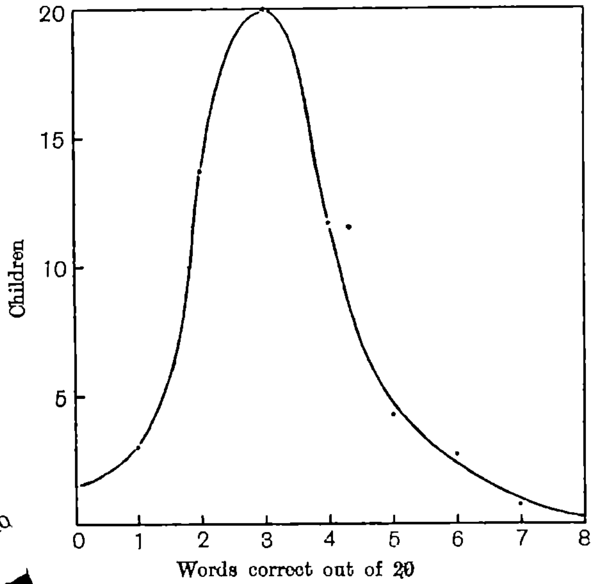

Fig. 2. Illustrating the scatter of the results.

It is clear from these considerations that the difference between boys and girls is not significant, though some weight must be attached to the fact that all the points but one on the boys' curve are above the girls' curve.

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OUTLINES OF A METHOD FOR THE QUANTITATIVE ANALYSIS OF WRITING VOCABULARIES.

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1. *Introduction.*
2. *Sampling Methods.*
3. *An Initial Proposition.*
4. *The Question of Weights.*
5. *Data from 'David Copperfield.'*
6. *One-, Two-, and Three-Group Solutions.*
7. *The Meaning of the Three-Group Solution.*
8. *The Outer Boundary of the Writing Vocabulary*
9. *Conclusion.*

1. INTRODUCTION.

THE present paper is an attempt to open out a way f[illegible] the size of the writing vocabulary of any person, and will [illegible] itself with the derivation and description of a method of attack, the further possibilities of which must be reserved for future treatment. The writing vocabulary is here distinguished from the reading and speaking vocabularies. A certain word may be intelligible to a person when it is heard or read, but yet may never appear in his writing. The writing vocabulary obviously differs more or less from both the others, and a comparison between the three will be of interest. The writing and speaking vocabularies of children and adults unaccustomed to verbal expression are undoubtedly much smaller than the number of words recognisable by them, either in print or when heard. A novelist or a public speaker, on the other hand, will use almost as many words as he recognises. Clearly some measurement of these different vocabularies is of interest to psychology, and not unconnected with the problem of the use of words in thinking, and with questions of imagery; and may be of use in pedagogy, for the unequal development of these different vocabularies is perchance at the root of some bad work and bad thinking.

It must be recognised at the outset that the attempt to assess any vocabulary can only be carried out in the face of numerous difficulties. A little reflection will show that the confines of a vocabulary are of a very vague nature. The acquisition of a word is not necessarily bound up with a single event, but may take place almost imperceptibly—the word seems to grow into the vocabulary and ripen there. At what moment then are we to say that it forms part of the vocabulary? Or, again, a word may be well known in its familiar meaning but may not yet bear other meanings which it carries in the minds of more educated persons; as when a boy knows the word *patent* in *patent medicine* but has no idea of its use in the phrase "it is *patent* to all observers that...."

Fluency in writing depends too on many things apart from mere [illegible]umerical extent of vocabulary. Powers of using various inflexions of [illegible]se, mood, degree, etc., powers of compounding words and of using [illegible]s in various metaphoric senses, powers of forming words of one part [illegible]ch from correlatives in another, all powers which render a vocabu[illegible]re flexible and mobile, must be recognised as elements of fluency. [illegible]rtant as all these matters are, they must at first be set aside if [illegible]itative measurement or comparison of vocabularies is to be [illegible] although it is not in the slightest degree suggested that they [illegible]nt, or not to be considered later.

[illegible] of counting words has therefore to be adopted which [illegible] elements and depends on what is virtually a definition [illegible]f a dist[illegible] and separate word. The method of dictionaries is to treat a gro[illegible] relative words as one paragraph with an elementary form [illegible]rd as a heading. This system is carried out in a good dictionary as consistently as possible and such a dictionary we have chosen as our arbitrary standard. That selected (for no very good reason except that it is one in common use) was Chambers's *Twentieth Century Dictionary*, which contains in round numbers some 35,000 label-words. These are taken as constituting the English language: and different compounds, inflected forms, etc., are considered as the same word if they are here given under one head. Of course, this is a very arbitrary procedure; it omits refinements of all kinds; and of these and all such defects no one is more aware than are the writers. But anyone who makes any quantitative statement about the size of a vocabulary must be employing, either consciously, definitely and scientifically, or unconsciously and loosely, some such standard. For example, "Max Müller estimated the total number of English words as 50,000; he pointed out that the ordinary speaking vocabulary of an ordinary English citizen, who reads his newspaper and books from Mudie's, does not extend beyond 3000 or 4000 words; that accurate thinkers and persons of wide knowledge probably use twice as many; that the Old Testament contains 5642 different words; that in all Milton's works you will find only about 8000; and that Shakespeare, who displayed a greater variety of expression than probably any writer in any language, produced all his works with 15,000 words. And at the same time he tells us that an uneducated English peasant lives and dies with a vocabulary which scarcely extends beyond 300 words[1]."

Such statements can have no meaning unless a definite understanding exists as to what are and what are not different words.

2. SAMPLING METHODS.

Having decided then to define the constituent words of a vocabul[illegible] in this way, the investigator may turn to the actual counting of voc[illegible] laries. Here there are two possible plans open to him. He may [illegible] has sufficient time and patience, examine every word ever wri[illegible] the author in question, and make a complete total of differe[illegible] found. The above statement for example, that the Old [illegible] contains 5642 different words, is presumably based on s[illegible] haustive count.

But as a rule this proceeding will be too long, and wh[illegible] a method of sampling. That is we wish to count a rea[illegible] sample of words and from them make an estimate of the whole number which estimate will be more and more reliable as the sample is increased in size.

Such a sampling method can easily be applied to the recognition vocabulary[2]. We choose a number of words from the dictionary by chance. The subject is tested and shows that he understands say sixty per cent. of them, whence we deduce that he probably understands sixty per cent. of the total number of words in the dictionary. It is not so easy however to decide from a sample of any person's writing how large is the writing vocabulary from which he is drawing the words he uses. The plan ordinarily adopted for a rough comparison is to note any very apt or very unusual words, and to judge from these. On this basis an experienced reader can undoubtedly say which of two writers appears to possess the larger vocabulary. But this is not a method lending itself to measurement, and for this reason the writers have developed the plan explained in the remainder of this article. It is in brief as follows. As more and more lines of an author's writing are examined, the number of new words met with becomes less and less, and from this rate of decrease something can be calculated of the size of the stock from which these new words are drawn.

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[1] Quoted by Sir Joshua Fitch, *Lectures on Teaching*, Camb. 1902 ed. page 226.

[2] See the preceding article.

3. AN INITIAL PROPOSITION.

Let us suppose, in the first place, that the extent of the writing vocabulary of a person is expressible by a number such as 10,000 or 20,000 words as the case may be. This number will represent the original stock or fund of words from which single words may be chosen at will to express thought through the medium of writing. The idea
a source of supply needs immediate qualification, since the use of
ds involves no diminution of the original stock. When a certain
appears the writer is still capable of using the same word again
many times as he pleases. This fact may be expressed in mathe-
terms by stating that an infinite number of identical specimens
ord is contained in the original stock[1]. Now let us assume that
f these words is equally likely to be written by the author.
urse not at all an assumption in keeping with the facts, for
as *the*, *a*, *is* are frequently written, and moreover certain
favourites, and others are constantly occurring because
under discussion. But we make the simplest assumption
g, with the intention later of modifying it more and more
re nearly the natural conditions. With this hypothesis,
he number of different words in the vocabulary, and if
made from it at random, thereby revealing y different words, the most probable value of N is given by the equation

$$y = N\left(1 - e^{-\frac{x}{N}}\right) \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(1).$$

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[1] It is
e must remember, that the possibility of a word occurring for
d by the fact that it has appeared before. The first appearance
a piece of continuous writing seems to influence later appearances
s. An elementary rule of composition prejudices the immediate
nce of word, while, on the other hand, the first appearance assures for that
ord and its associations a livelier connection for some time with the elements
f attention

A proof of this theorem in both the exponential form here given and also the binomial form is given in an appendix. The curve representing equation (1) is shown in Fig. 1.

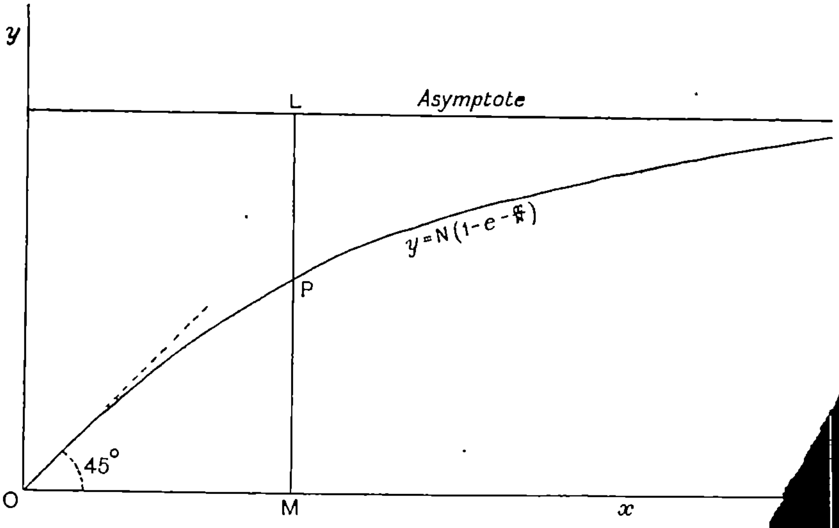

Fig. 1.

At any point P in the curve the ordinate PM represents the
of different words revealed, OM the number of selections made
height ML of the horizontal asymptote represents N, the to
lary. The curve begins with a gradient of 45° and is horizont
The general properties of this curve may be understood b
sideration of the nature of the problem. When a very sm
selections have been made each additional selection re
different from all the rest, hence the number of different
is as great as the number of selections made. Under
the gradient of the curve is obviously 45°. Such con
obtain when the number of selections is very small, for very soon
selections will be made which do not reveal new words and the numb
selections will from this stage onwards exceed the number of diff
words by an ever-increasing number. Hence the above cu
deviate more and more from the original gradient. The a
property of the curve is accounted for when the number
words approaches the extent of the original stock. At th
additional selection becomes less and less likely to reve
and hence the curve gradually approaches a horizontal

would signify that all available words have been discovered. Consider, as an illustrative example, a bag containing balls of 25 different colours, an equal number of each colour. If, after careful shaking, one ball is withdrawn, it may with equal likelihood be any colour. Return the ball and shake again and the condition for the next selection is the same as before. Repeat, a number of times, and record the results in tabular form. The following is a set of results from an actual experiment.

No. of selections ...	10	20	30	40	50	60	70	80	100	120
No. of different colours	8	15	18	20	21	22	23	23	24	25

Using the first row of numbers as abscissae and the second row as ordinates, a curve will be constructed agreeing with the theoretical curve of Fig. 1 (where $N = 25$) within the limits of experimental error. Any pair of values of x and y will serve for the calculation of N, the number of different colours in the bag. The probable error of the value of N [illegible]us found will differ according to the pair of values that are used in the [illegible]ulation. The higher the value for x, the smaller will be the probable [illegible] and hence N will be calculated the more accurately. For example, [illegible]en balls have been drawn, of which eight prove to be different, we

$$8 = N\left(1 - e^{-\frac{10}{N}}\right),$$

[illegible]. At this stage therefore we would be able to predict that the bag contained 22 kinds of balls. At a later stage our estimate would be more accurate.

4. THE QUESTION OF WEIGHTS.

The example we have worked out at the end of the last section shows clearly the method of sampling we wish to apply to writing vocabularies. After making only ten dips into a bag of balls and finding eight different colours we are able to predict that there are probably twenty-two different colours altogether, a prediction not so very far from the truth. In the same way by examining a comparatively small number of words in a novel and finding how many of them are different we hope to be able to predict the size of the writer's vocabulary. But for reasons already given on page 55, it is clear that the problem of a person's writing vocabulary is not nearly as simple as that of the above example.

A nearer approach to the actual mental conditions is made when we consider that each word has a relative likelihood of appearance as compared with other words, according to the composition of the writing vocabulary. The chance of appearance of a particular word will be afterwards described as the weight of the word, and may be expressed by a simple number, which will decide, in the long run, the relative frequency of appearance of the word[1]. Each word will then have its own particular weight in the vocabulary of any one person, and the weight of a word will differ with different persons. Since the values of the weights of words are purely relative any scale may be preferred, but it seems satisfactory to regard the lowest weight considered as unity. In practice, a number of words will have approximately the same weight, and will form a group. Following this suggestion, a vocabulary may be regarded as being composed, more or less accurately, of a number of groups of varying sizes each possessing a common weight.

The inadequacy of the assumptions in the initial proposition is now evident. In that case all words were considered as having the same weight—a sweeping assumption, which is obviously far from the trut
The introduction of group formations with characteristic weig
modifies the proposition to an unlimited extent. A correspon
modification of equation (1) must now be derived. For this pu
imagine a person's writing vocabulary to consist of n_1 words each
the weight w_1, n_2 words each having the weight w_2, and so o
number of groups. Let x selections be made at random ou
a vocabulary. The most probable number of selections wh
made in the first group is

$$\frac{n_1 w_1}{\Sigma nw} x,$$

where $$\Sigma nw = n_1 w_1 + n_2 w_2 + n_3 w_3 + \ldots \text{ etc.}$$

In general, the most probable number of selections which will be made in the rth group is

$$\frac{n_r w_r}{\Sigma nw} x.$$

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[1] Some difficulties arise out of the adoption of a number to denote the weight of a word, for this is not a fixed quantity. Not even in the vocabulary of the same person can a certain word always have the same weight. In the fluctuations of experiences, a word may increase or decrease in favour and the weight will correspondingly fluctuate. Further, a word cannot maintain the same weight in the writing vocabulary regardless of topic or circumstance. On a particular occasion the weight of a word is partly decided by its relation to the subject-matter requiring expression. Throughout the course of a novel, for instance, the weight of a word will vary from chapter to chapter or even from page to page. This difficulty must be faced later.

Consider the selections in the first group alone, applying equation (1), and let y_1 represent the number of different words revealed in this group. Then the most probable value for y_1 is given by

$$y_1 = n_1 \left(1 - e^{-\frac{w_1 x}{\Sigma n w}}\right).$$

And in general

$$y_r = n_r \left(1 - e^{-\frac{w_r x}{\Sigma n w}}\right).$$

Now let y represent the total number of different words revealed in all the groups. Then

$$y = n_1 \left(1 - e^{-\frac{w_1 x}{\Sigma n w}}\right) + n_2 \left(1 - e^{-\frac{w_2 x}{\Sigma n w}}\right) + \ldots + n_r \left(1 - e^{-\frac{w_r x}{\Sigma n w}}\right) + \ldots \ldots (2).$$

This is the general equation[1], which, if the required number of groups is considered, will satisfy any person's writing vocabulary. The question of the probable error is vital to the whole of the above argument, and will be touched on presently, though its complete consideration must be deferred to a later paper.

DATA FROM THE WRITING VOCABULARY OF CHARLES DICKENS.

a preliminary application of the foregoing, it was decided to collect material from the writing vocabulary of Charles Dickens. d necessary to draw from a copious supply of material, not restricted in style and topic. For this a novel by Dickens ive ample scope, and the first attempt was made from iting in chapter LV of *David Copperfield*.

The ous results accruing from the counting of 4000 words are ven able I. The first column gives the total number of words counted, while the corresponding numbers in the third column give the mean value for the number of different words found in each case[2]. The separate numbers in the second column are in a few cases not absolutely independent of each other. Two of the numbers for instance which give the results of 1000 selections are not taken from two separate 1000 words, but have the same 600 words common to both groups[3]. The contents of the fourth column will be explained later.

It is interesting to note the agreement between the numbers in the second column. The results for selections of 200 words afford the greatest opportunity for examination. The numbers for selections of 2000 words are in striking agreement, especially when it is considered that two entirely different sets of words (2000 each) were counted in this case. Even in the case of 3000 words, where the difference of 20 is larger than usual, the discrepancy is only about 2·6 per cent.

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[1] For fuller explanation of the general equation see Appendix II.

[2] Proper nouns were omitted from the count.

[3] A more methodical system of counting and recording will obviate any need for this.

TABLE I.

No. of words examined	No. different in each separate case	Mean no. different	Calculated no. different
50	42, 40	41	42
100	77, 73	75	74
150	98, 101	99·5	100
200	104, 118, 116, 124, 131, 120, 114, 119, 124	119	122
300	167, 172	170	161
400	194, 212, 204, 205	204	196
600	265, 254, 267, 266	263	260
800	324, 301, 328, 325	320	318
1000	387, 383, 392, 375	384	369
1200	436, 442 417	432	423
1600	514	514	514
2000	591, 592	592	594
2600	691, 699	695	701
3000	772, 752	762	765
3600	852	852	854
4000	902	902	90

When we think of the freedom in the choice of term and ex and the apparent vagueness of such a thing as a writing both in extent and construction, the proximity of these column 2 to each other excites some degree of surprise.

6. ONE-, TWO-, AND THREE-GROUP SOLUTIONS.

The nature of the problem of establishing solutions upon the above data is most clearly understood by reference to the curve of distribution. Fig. 1 shows the form of the curve when unmodified by weights. In the case of a person's writing vocabulary all that can be known, except by very prolonged counting, is the earlier part of the curve. From this information, it is desired to find, if possible, an equation for the complete curve, estimating thereby the nature of the group formations within the vocabulary, and if possible the height of the horizontal part of the curve, which would represent the total vocabulary.

(*a*) *One-Group Solution.* By assuming that the vocabulary is made up of a single group of words with a common weight we have etition of the initial proposition. Equation (2) reduces to the ity of equation (1) and we are then dealing with the pure curve Since only one pair of ordinates is necessary to fix such a result from the table would in general give a different curve different solution. For example, if we use the ordinates which express the fact that in 2000 words 592 were different, stituting in the equation

$$y = N\left(1 - e^{-\frac{x}{N}}\right)$$

lue $N = 615$.

e curve is therefore

$$y = 615\left(1 - e^{-\frac{x}{615}}\right),$$

vocabulary on this hypothesis is 615 words.

n a similar way from the ordinates (4000, 902) we get e total vocabulary. That is the hypothesis gives different olutions with each pair of ordinates and moreover it is obvious that hese results are far too small, and any presumption that they indicate extent of a vocabulary is ridiculous. The error lies in the assumption all words are of equal weight.

) *Two-Group Solution.* An advance can be made by assuming p formation of the second order. Let the vocabulary be made wo groups containing n_1 and n_2 words respectively, and let d in the first group have weight w_1 and each word in group have weight w_2. Let w_1 be the smaller weight, and fix the scale let it be unity. Since three quantities now e found, three pairs of co-ordinates are necessary and ield a solution. Moreover, although a different solution d for every different set of values chosen for calculation sistencies are not now so striking. A more reliable two-group solution is that which gives a curve passing most approximately among all the points. Such a solution possesses a higher degree of probability.

In order to illustrate the success of the two-group method an approximate solution was found from seven pairs of co-ordinates. In this solution the values for n_1 and n_2 are 1750 and 120 respectively, while w_2 has a value 40. The following table gives a test of the accuracy of the solution.

TABLE II.

Number of words examined	Number of different words	Probable number of different words calculated from the two-group solution
200	119	137
400	204	213
600	263	270
1000	384	368
2000	592	581
3000	762	763
4000	902	920

The numbers in the first and second columns are results of actual countings while the numbers in the third column are the values for y calculated from the formula

$$y = 1750\,(1 - e^{-\frac{x}{6550}}) + 120\,(1 - e^{-\frac{40x}{6550}}),$$

x having in turn the values in the first column. A comparison of t [illegible] second and third columns reveals the character of the solution. Wh [illegible] being fairly near it cannot be called satisfactory. The vocabula [illegible] indicates consists merely of 1870 words (1750 + 120), obviou [illegible] below the actual vocabulary. The solution is chiefly interes [illegible] showing the results obtained by the first and simplest introd [illegible] weights, and its superiority over the one-group solution po [illegible] right direction.

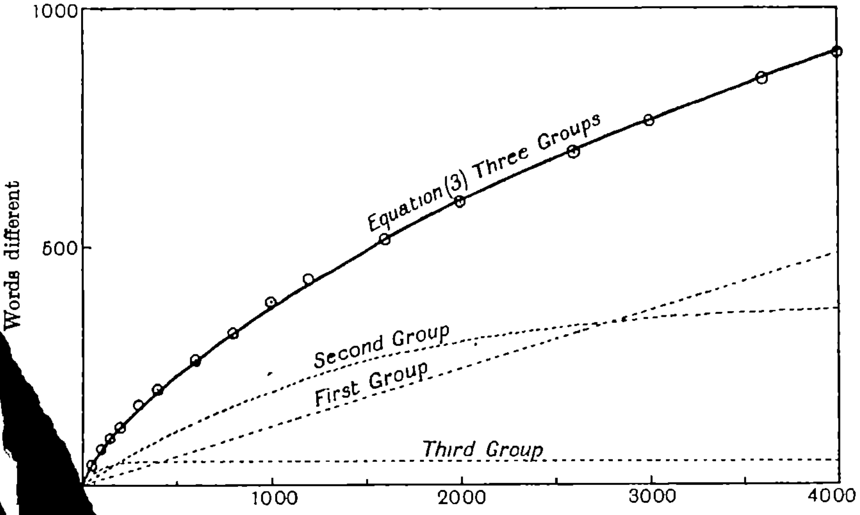
(c) *Three-Group Solution.* The next approximation [illegible] conditions in the writing vocabulary involves the assum [illegible] groups of words. Fixing the value of w_1 as unity the as [illegible] to five unknowns, namely, n_1, n_2, n_3, w_2, and w_3, requi [illegible] of co-ordinates to give one solution. An attempt has aga [illegible] however, to find a solution which will most approximately fit the whole of the data of Table I. In this solution the sizes of the three groups and their relative characteristic weights are as follows:

$n_1 = 7300$, $w_1 = 1$ (but see page 65)
$n_2 = 400$, $w_2 = 40$
$n_3 = 50$, $w_3 = 700$

The three-group equation therefore becomes

$$y = 7300\,(1 - e^{-\frac{x}{58,300}}) + 400\,(1 - e^{-\frac{40x}{58,300}}) + 50\,(1 - e^{-\frac{700x}{58,300}}) \ldots (3).$$

The fourth column in Table I contains a series of values for y calcu[illegible]ted from this formula, for comparison with their corresponding [illegible]xperimental values. The most serious difference between calculated and experimental values is in the case $x = 1000$, where a discrepancy of nearly 4 per cent. is noted. Many of the other differences are almost negligible.



Words examined

Fig. 2.

[illegible] presents in graphic form the comparison between experi- [illegible] alculated values. The individual points lying near the [illegible] rimental, while the smooth line is the curve of equation [illegible] imity of the points to the smooth curve displays the [illegible] e three-group solution within the range considered. The [illegible] of the curve of equation (3) is seen from the three constituent curves which are included in the same figure. The curve obtained by selections from the third group shows that its supply was exhausted after a selection of 400 words from the vocabulary, whence its extremely high characteristic weight. The curve derived from the second group shows nearly the whole of its deflection within the range of the figure and is nearly exhausted by the selection of 4000 words from the vocabulary. The first group shows only the slightest signs of deflection within the given range, but its production beyond the figure [illegible] bending of the same nature as in the other two groups, although a very much larger number of selections would be required to bring it to the stage of exhaustion. Although this theoretical curve passes very closely through the experimental points, yet it does not do so exactly. But by increasing the number of groups into which we have divided our vocabulary we would presently arrive at a point where the number of algebraical constants equalled the number of experimental values, and the theoretical curve then drawn would fit the data exactly. The algebraical labour required to obtain this solution, however, would not be repaid, for the improvement in apparent fit would not be worth while when the probable errors of the experimental points (due to sampling) were taken into consideration. A three-group or perhaps a four-group solution is as good as the data will allow.

7. THE MEANING OF THE THREE-GROUP SOLUTION.

We have found then that the Tempest chapter of *David Copperfield* is, as far as the actual number of different words goes, consistent with the assumption that the vocabulary from which Dickens was drawing while writing it was composed of 7750 words in all, of which 7300 h[illegible] a certain chance of being selected, 400 have forty times that ch[illegible] and 50 are used very frequently indeed, having a weight of no les[illegible] 700. These conclusions we came to from an examination of 400[illegible] of which 902 were different.

But we do not wish for one moment to make any asse[illegible] these numbers yet represent the whole of Dickens' actual [illegible] The limitations to which they are subject we wish to [illegible] this section of the paper. In the first place they wou[illegible] only represent Dickens' 'Tempest' vocabulary and man[illegible] Dickens employs elsewhere would never be used even [illegible] write for ever on the subject of this chapter. This ho[illegible] esting in itself as opening up a possibility of compar[illegible] vocabulary on various subjects. If it were desired to apply our meth[illegible] to the whole vocabulary and not to any section of it, the words coun[illegible] ought to be chosen at random from all available writings of the aut[illegible] and not in continuous order. The curve then obtained would undoubtedly be much straighter, and the asymptote would be higher, since the total vocabulary would be greater than any subject vocabulary.

But even regarded as representing a certain subject vocabulary these numbers lack definiteness. The fifty words which form the third group in the above solution and are of such very frequent occurrence, are chiefly words such as *the*, *and*, *be*, *to*, *of*, and so on, words necessary to the cohesion of any piece of writing. The 400 words of the middle group of medium frequency are partly further words of the above class, less necessary but yet often employed; partly words connected with the subject of the chapter, words like *ship* and *storm*, *beach* and *shore*, *wreck* and *sail*. The size of these two groups is settled by the solution of the equations in a fairly definite manner: any serious alteration in n_2 or w_2, n_3 or w_3 sends the theoretical curve off the experimental points, though even here the limits are far from narrow. But it is otherwise with the first group of 7300 words of small weight. Here enormous changes in n_1 and w_1 can be made without appreciably affecting the solution, for this curve, unlike the others, is practically a straight line up to 4000 words. As long as the product $n_1 w_1$ is kept constant and w_1 not allowed to rise too much, the curve of the first group in Fig. 2 is practically unchanged and the other two curves are untouched. For example, instead of 7300 words with unit weight we might substitute 14,600 words with weight *one-half*, or 3650 words with weight *two*[1]: or we may replace this single first group by two or more groups, provided we keep the weights low and Σnw constant.

In other words, there is no definite boundary indicated to this large group; we can only say how big it is if we arbitrarily fix its weight in comparison with the other weights. We can fix a lower limit to n_1, for if w_1 is increased much beyond unity the first-group constituent curve begins to bend and throws the complete curve off the experimental points. But we can fix no upper limit except that the total vocabulary cannot, by our dictionary definition of vocabulary, exceed 35,000 words. This difficulty will decrease as the sample of words is enlarged, but 4000 is already as much as one cares to count. For writers (children for example) whose vocabulary is much less than that of Dickens, 4000 words would give a more definite solution, for the sample would be relatively greater.

8. THE OUTER BOUNDARY OF THE WRITING VOCABULARY.

It would seem then that our method fails to give the size of the writing vocabulary, although it gives with some accuracy the size of the more frequently used groups: it contours the vocabulary, as it were, and does it rather well near the summit but becomes vague near the sea-coast. But in this respect it seems to us that our solution is in

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[1] The weight of a word, it must be remembered, is a purely relative quantity

accordance with facts, for the outer boundary of a vocab
truth vague, and there is a limbo of incompletely knc
surrounding a core of those which come readily from the
vagueness is no doubt greatest and most obvious in the
vocabulary, where it becomes a matter of nice judgmen e
whether a word is known or not, that is, to decide where he
boundary. But also in the writing vocabulary, though here ue-
ness is less, the difficulty occurs, and it is hard to say wheth now
a word well enough to be likely to use it in composition. T exactly
what is represented by the weight of the first group. Our tion tells
us that the second and third groups are so and so big a have such
and such relative weights. But of the first and largest oup it only
tells us the product of n_1 and w_1 on the scale of weighting chosen, and leaves us then to decide at which weight we draw the boundary.

If we say for example that we will arbitrarily draw the boundary at a point such that (in a three-group method) the weight of the largest group is one-thousandth of that of the smallest, then we can speak definitely of the size of the whole vocabulary. It will be

$$\begin{array}{ll} n_1 = 10{,}430, & w_1 = 0{\cdot}7 \\ n_2 = 400, & w_2 = 40 \\ n_3 = 50, & w_3 = 700 \end{array}$$

and $N = n_1 + n_2 + n_3 = 10{,}880$ words.

If we keep to this or some other standard, and if expressions for the probable errors can be given, comparisons can be instituted between different authors or different subjects. This the writers are attempting in other papers.

9. CONCLUSION.

We have tried in this paper to indicate a sampling method of estimating the size of writing vocabularies, and of analysing the vocabulary into groups of words of more or less frequent use. The task here commenced leads to a long inquiry which may turn out to be fruitless; for the probable errors are undoubtedly large and the size of sample required may prove enormous. But even in that case it may be of use in exposing the vagueness of ordinary estimates of vocabulary, while if the method should prove to be practicable it would open up an interesting province of research. In particular the writers hope to apply it to a comparison of the recognition and expression vocabularies of children.

APPENDIX I.

PROOF OF THE INITIAL PROPOSITION.

1. *Binomial Form.*

Let N = no. of different objects, all of which have the same chance of being selected on any occasion.

x = no. of selections made.

y = no. of different objects revealed after x selections.

Consider by two distinct and separate methods the chance of any one object remaining unchosen.

(*a*) The chance of an object being chosen in one selection is $\frac{1}{N}$.

The chance of an object being unchosen in one selection is $\left(1-\frac{1}{N}\right)$.

The chance of an object being unchosen in x selections is $\left(1-\frac{1}{N}\right)^x$.

(*b*) After x selections have been made, y different objects are revealed and $N-y$ therefore remain unchosen.

The chance of any object remaining unchosen is therefore $\frac{N-y}{N}$.

Two expressions are now found for the same quantity, and equating these we get:

$$\frac{N-y}{N}=\left(1-\frac{1}{N}\right)^x,$$

whence
$$y=N\left\{1-\left(1-\frac{1}{N}\right)^x\right\}.$$

2. *Exponential Form.*

Let N, x, y have the same meaning as before, and in addition let y' = the number of objects remaining unchosen. Then

$$y+y'=N.$$

If after x selections an additional selection is made, the chance of a new word being revealed is $\frac{y'}{N}$.

Hence $\frac{y'}{N}$ is the decrease in y', produced by unit increase in x. Therefore assuming the possibility of infinitesimal increments,

$$\frac{dy'}{dx}=-\frac{y'}{N},$$

whence
$$y'=Ke^{-\frac{x}{N}}.$$

Using the initial conditions to fix K, we find, when $x = 0$,

$$y' = N = K,$$

$$\therefore\ y' = Ne^{-\frac{x}{N}},$$

$$\therefore\ y = N - y' = N(1 - e^{-\frac{x}{N}}).$$

This form is more convenient for calculation than the binomial form and comes in practice to the same thing. The condition implied is that N is large, and $\frac{x}{N}$ not large, compared with unity.

APPENDIX II.

DETAILED PROOF OF THE GENERAL EQUATION.

Let n_1, n_2, n_3, etc. represent the actual number of words in each group respectively, and let w_1, w_2, w_3, etc. represent the relative weights of each word in the corresponding groups; of these weights let w_1 be the least and have unit value. In Fig. 3 (*a*) each column represents a group of words. Let the height of the first column be n_1, the height of the second n_2, and so on, and let the base of each

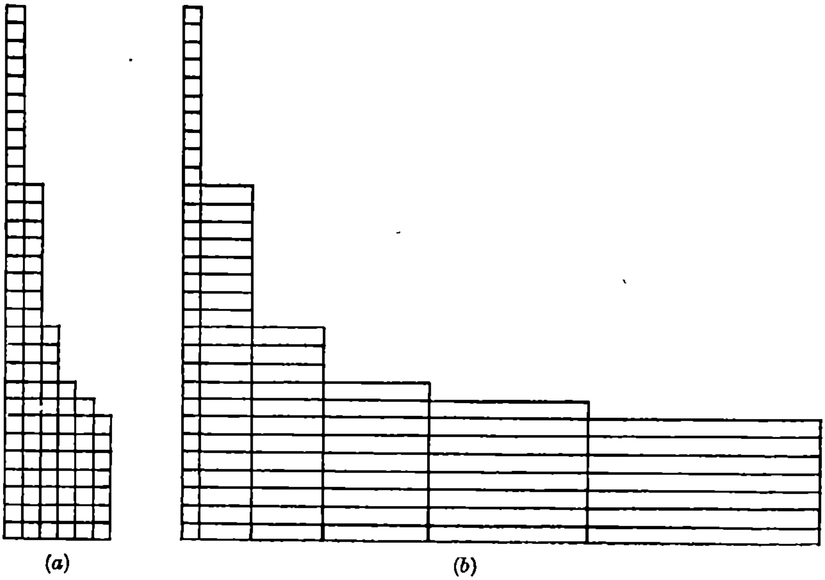
(*a*) (*b*)

Fig. 3.

column be of unit length. Each column is therefore made up of as many unit squares as there are words in the group it represents. The area of the whole figure is $\Sigma n = N$, the total vocabulary. We may then think of each separate word occupying its own particular square and the act of choosing a word may be compared with a shot piercing the square which represents that word. In order to allow for weights the figure must be modified, so that a word of weight 10 may possess ten times the chance of being reached by a random shot. Fig. 3 (*b*) will satisfy these conditions, where the height of each column is the same as before but the base has expanded to a length representing the weight of each group respectively. A word of weight 10 for example now occupies a horizontal row equal to 10 squares. The area of this figure is Σnw.

Now let the area enclosed by Fig. 3 (*b*) be subjected to a hail of random shots. Let x shots in all fall within the whole area. Then the most probable number of shots to strike the rth column is

$$x_r = \frac{\text{Area of } r\text{th column}}{\text{Total area}} \times x = \frac{n_r w_r}{\Sigma nw} \times x.$$

Within this column each row has an equal chance of being struck so that to find the number of rows pierced at least once we can apply equation (1)

$$y_r = n_r (1 - e^{-\frac{x_r}{n_r}}),$$

which on substituting for x_r becomes

$$y_r = n_r (1 - e^{-\frac{w_r x}{\Sigma nw}}).$$

This gives the number of different words y_r which are revealed in the rth column. The total number of words revealed is therefore

$$y = y_1 + y_2 + \dots + y_r + \dots \text{ etc.}$$

$$= n_1 (1 - e^{-\frac{w_1 x}{\Sigma nw}}) + n_2 (1 - e^{-\frac{w_2 x}{\Sigma nw}}) + \dots \text{ etc.}$$

If we extend the system of weights until every word is given its own particular weight, this becomes

$$y = \Sigma (1 - e^{-\frac{wx}{\Sigma w}}),$$

where each summation is over the whole vocabulary of N words.

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FACTORS IN THE MENTAL PROCESSES OF SCHOOL CHILDREN[1].

II. ON THE NATURE OF THE SPECIFIC MENTAL FACTORS[2].

By N. CAREY.

(*From the Psychological Laboratory, University College, London.*)

1. *Theories and Problems.*
    - (i) *Introduction.*
    - (ii) *Formal Training Theory.*
    - (iii) *'Content' Theory.*
    - (iv) *Theory of a general factor.*
    - (v) *Problems.*
2. *Methods.*
3. *Previous Researches.*
    - (i) *By the 'transference' method.*
    - (ii) *By the correlational method.*
4. *The Experiments.*
5. *Results.*
    - (i) *Procedure.*
    - (ii) *Correlational groups of tests of similar act but different content.*
    - (iii) *Correlational groups of tests of similar content but different act.*
    - (iv) *Other groups.*
    - (v) *Conclusions.*

1. INTRODUCTION.—THEORIES AND PROBLEMS.

(i) A COMMON view of the constitution of our mental life is that knowledge derived through the medium of the senses is made use of by certain general activities of the mind, such as memorising, discrimination, observation, reasoning, etc. These general activities are not regarded as being entirely independent of each other; one may involve another. Thus, for example, success in discrimination is dependent upon the degree of active attention. Training in one activity will also result in the training of any other activity involved. Thus in training the power of memorising, other general activities such as attention, observation, volition, etc. are trained.

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[1] Thesis approved for the degree of Doctor of Science in the University of London

[2] For Section I see this *Journal*, VII. Part 4, 453.

According to this view psychoses may be classified according to sense material (content) or according to mental activity (act). From a classification according to content—visual, auditory, kinaesthetic, etc. —results the classification of individuals as eye-minded, ear-minded, muscle-minded, etc. From a classification according to the form of operation or the mental act involved—discrimination, memorising, reasoning, etc.—individuals are described as possessing good powers of discriminating, memorising, reasoning, etc.

(ii) Methods of training have varied according to the mental classification recognised. For many years certain subjects have been taught because they involve certain of the general activities, such as reasoning or observation. The value of the content as such has been ignored, it being assumed that if a general activity is improved with regard to one content, it will also be improved with regard to another. (Formal training theory.)

(iii) Within more recent years schools have arisen where the value of the subjects taught is judged according to content and not according to the mental operation involved. In these schools it is maintained that there is no transfer of training from one content to another, that "one thing cannot be taught by teaching another," and that therefore the subjects for instruction must be chosen for the usefulness or desirability of their content. ('Content' theory.)

(iv) Still more recently another theory has been brought forward which, although quite different in many respects from the other two, yet from certain points of view may be regarded as intermediate between them. This theory posits the existence of a general factor (g), which has been conceived as a fund of "intellective energy[1]" available in all the higher mental processes. An individual possessing a large fund of "intellective energy" will give signs of high general ability. The general factor, however, will not explain all mental phenomena. It is often found that children of recognised high general ability fail to do themselves justice in individual subjects such as music, drawing or arithmetic; while children of low general ability often excel in one particular subject. The theory of a general factor must therefore be supplemented by a theory of specific factors.

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[1] Hart and Spearman. "General Ability," this *Journal*, v. Part I.

(v) Already a considerable number of investigations have been made in order to discover the best means of measuring individual general ability. But in order to make the best use of any such knowledge for educational purposes, it is also necessary to know as much as possible of individual specific abilities. Before, however, we are able to make any quantitative measurements of specific abilities, they must be investigated from a qualitative point of view, that is, their nature, range and relations must be determined. Are they of the nature of act, or content, or both? If of act, then the formal training theory still holds good; if of content, then the materialistic theory; if of both, both theories are partly true, a point of view put forward by Prof. Spearman in his paper on "Qualified and Unqualified Formal Training[1]."

The present section is an endeavour to gain some knowledge of the nature of specific mental abilities.

2. METHODS.

Many classifications used in psychology are merely descriptive ones, and it does not follow that because two mental processes come under the same heading there is necessarily a functional relation between them. Thus, for example, the seeing of a colour, and the hearing of a tone, will both come under the heading of perception, but it cannot be asserted on this account that an adept at colour perception will be an adept in perceiving tones. A judgment concerning the length of a line, and a judgment with regard to the length of time the war is likely to last, may for descriptive purposes come under the same heading, namely judgment, but it does not follow that an individual who judges correctly in the first case will also do so in the second.

In order to determine the existence of a functional relation or specific factor between any two abilities, two methods may be employed. The first method, the dynamic, is that of determining whether practice in one ability brings about improvement in the other. If practice in the one results in improvement in the other, a functional relation or specific factor exists between the abilities. The extent of the relationship or the size of the specific factor will be indicated by the amount of transference.

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[1] *Jour. of Exp. Ped* 1914.

The second method, the static, is that of determining the degree of correlation between the two abilities[1]. A high correlation, however, cannot at once be interpreted as indicative of the existence of a functional relation or specific factor. A positive correlation may be due to the general factor, a specific factor, or to both. On the other hand, zero correlation always gives information concerning both factors, that is, that they are both non-existent. In this second method then, before drawing any conclusions, it is necessary to eliminate *g*.

3. PREVIOUS RESEARCHES.

(i) Not until quite recently have experimental investigations been made concerning the various theories upon which educational practice has been based. Of those which have been carried out a large proportion have been concerned with the transference of training in the domain of memory. A detailed and critical account of the most important of these is given by Dr Sleight in "Memory and Formal Training" (this *Journal*, 1911, p. 389). Meumann and Ebert, Fracker and Winch had all found transference of training from the memorising of one kind of material to the memorising of another kind. From these results it had been concluded that there is something of the nature of a general memory function, a specific factor which is not content. Dr Sleight, after criticising these researches, made another investigation into this same question (p. 402). His results, however, did not corroborate those of the above-mentioned investigators. He found that instead of a general memory function, there is a large number of unrelated memories. Transference only occurs when there is some common separable element, such as specific forms of attention, imagery, rhythm. Small differences in procedure may bring about a great loss of transfer. Very briefly, according to Dr Sleight's results, there is no general memory function, and common factors which may occur in the various special memories are very limited in their effects, and may be either of the nature of act or content.

(ii) The number of investigations into the nature of specific abilities by the method of correlating the results of tests is small. An account of the earlier of these is given by Prof. Spearman in "General Intelligence," *Amer. J. of Psych.* 1904, pp. 206–219, and as he points out, all the results except those of Wissler are valueless owing to the lack of an adequate system of investigation. Not only was there no method of determining the value and accuracy of a correlation, there was none for discriminating between the general factor and specific common factor. It is only during the last few years that suitable mathematical methods have been available for the treatment of any results obtained. Up to the present these methods have chiefly been used in order to establish the existence of a general factor and little attention has been paid to specific factors.

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[1] Concerning static and dynamic correlation see C. Spearman, "General Intelligence," *Amer. J. of Psych.* 1904, 216.

In 1909 Thorndike published some correlations obtained between accuracy of sensory discrimination and general intelligence[1]. There were two discrimination tests which measured accuracy in drawing lines and accuracy in making up weights. The correlations of both these tests with general intelligence were low, namely ·15 and ·25 respectively, but the correlation between the two tests themselves was ·50. Before the carrying out of these tests Thorndike was of the opinion that the mind is the sum total of countless particular capacities, each of which is to some extent independent of the others, and must be to some extent educated by itself[2]. Afterwards, however, he suggested that there are three "levels" of mental activity, namely, sensitivity, association, and analysis[3]. Performances belonging to the same level would correlate more highly than performances belonging to different levels. His reason for thus changing his view is that, in the case of the tests mentioned above, the correlation between the tests themselves is much higher than either of them with general intelligence[4]. He also quotes another instance in which the inter-correlation between efficiency in marking out *A*'s, the finding of circles, hexagons or isosceles triangles among geometrical figures, the finding of misspelled words, are higher than their correlations with general intelligence. Although performances belonging to the same level correlate more highly than performances belonging to different levels, Thorndike points out that such correlation is not necessarily very high, particularly in the sensitivity group. That which causes correlation between two performances of the same level is of the nature of act; for Thorndike says that any one of the sensitivities "would relate very loosely to the associative or analytical functions, even when busied with data from the same sense." Similarity of content is evidently not important.

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[1] Thorndike, Lay and Dean, "The relation of accuracy in sensory discrimination to general intelligence," *Amer. J. of Psych.* 1909, 368.

[2] *Principles of Teaching*, Chapter on Formal Discipline.

[3] *Educ. Psych.* 1910, 191.

[4] The higher correlation between these two tests may be due to the motor factor, the existence of which has been proved with regard to the school subjects.

With regard to the general factor, Thorndike absolutely denies its existence on account of the low correlations of general intelligence as estimated by teachers with any of the tests which have been mentioned, even after the correction formulae of Prof. Spearman have been applied[1]. These low correlations alone, however, cannot be taken as proving the non-existence of the general factor. Suppose any two performances actually to involve *g*, the correlation between them will not be 1·00 until the influence of all specific factors has been eliminated.

4. THE EXPERIMENTS.

The research of which an account will now be given was carried out in five classes of the senior department of a London County Council elementary school. Four classes were of girls, and one of boys.

The tests used fall into six groups. The first group consists of seven tests in discrimination of (1) the pitch of musical notes, (2) the rate of ticking of a metronome, (3) musical phrases, (4) various shades of yellow, (5) the size of angles, (6) patterns, (7) groups of colours. The second group consists of memory tests in the same seven kinds of sense material. In the third are three verbal memory tests; the first auditory, the second auditory-motor, and the third visual. The fourth group comprises the school subjects, marks for each of which were obtained from weekly and terminal tests. In the fifth group are four estimates of the scholastic intelligence, practical intelligence, painstaking, and social status of the children tested, drawn up by two or more teachers. The sixth group consists of two tests calculated to measure the general factor. In the first a number of words were read off at a given rate, and the children were required to write down their opposites. In the second they were required to rearrange certain words into sentences. For the details of the tests in these six groups, the reader is referred to the paper on "Visual and auditory imagery," this *Journal*, VII. p. 460.

In addition to the tests mentioned above, three other verbal memory tests were given. In the first of these three tests, four series of ten words were drawn up. The first series consisted of the names of foods, the second of articles of dress, the third of articles of furniture, and the fourth of animals. These series were read out at a given rate, the children repeating each word after the experimenter. Each series was read three times before passing on to the next. At the end of the fourth series, the children were asked to write down as many of the first series as they could remember, and then similarly of the second, third and fourth series in order. It should be noted that it was found very difficult to obtain reliable results in this test. After three repetitions the reliability was only ·42. It will be referred to as the associated verbal memory test.

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[1] Thorndike, Lay and Dean, *Op. cit.* 367. C. Spearman, "General intelligence objectively determined and measured," *Amer. J. of Psych.* April 1904, XV. No. 2. See also Thorndike, *Educ. Psych.* III. 363–5.

The second test was a test of memory of meaning. A short story was read through twice, the children repeating it. At the end of the second repetition they were asked to write out as much of it as they could remember. This process was repeated four times in order to obtain reliable measurements.

The material for the third test consisted of series of words (concrete nouns) with particular numbers attached to each word. The words with their numbers were read three times, the children repeating, but with each repetition the order of the series was varied. At the end of the third reading, the experimenter read the words in a certain order, one at a time, while the children were required to write down the number attached to each word. The test was repeated with six series of increasing difficulty; the first contained three words, the second and third, four; the fourth and fifth, five; and the sixth, six words. This, for want of a better name, will be called the mechanical memory test.

Besides the seven sensory discrimination tests, three tests in the discrimination of the thickness of wire, the size of balls and the weight of Galton cartridges were carried out. The three sets of marks obtained were pooled and the results are given under the heading of tactile discrimination. The corresponding memory tests were carried out, but as these showed an extremely low reliability coefficient the results will not be given.

These three verbal memory tests and the tactile discrimination test were only carried out in two classes, namely, standards II and V.

5. RESULTS.

(i) *Procedure.* At the completion of the experiments, all tests for each class were inter-correlated and finally the results of the five tables thus obtained were averaged to form one table[1]. This table is not given completely, but has been divided into various groups 1–13, in order to facilitate examination. All the coefficients in each group are corrected for unreliability.

In the remaining tables which are given, the tests in certain of the groups have been pooled. The details of the pooling are given on p. 483 of the paper on "Imagery." In Table XIV are the inter-correlations between these pooled tests. In considering the individual correlations in this table, it is desirable to determine how much of each correlation is due to the general factor, and how much to specific factors. The correlation of each test with g was determined by means of the formula

$$r_{xg}^2 = \frac{r_{xa} \cdot r_{xb}}{r_{ab}},$$

where x is the performance whose correlation with g is desired, and a and b are two performances whose correlation with one another and with x is due to g only[2]. In working out each coefficient, as many pairs of tests as possible corresponding to a and b were made use of, and the average result taken as the true one. Thus in determining the correlation between auditory memory and g the following pairs of tests were used: opposites and drawings, opposites and holes, sentences and drawings, sentences and holes, scholastic intelligence (S.I.) and holes, S.I. and opposites, S.I. and drawings, S.I. and sentences, verbal memory and sentences, verbal memory and drawings, verbal memory and holes, verbal memory and S.I.—altogether twelve pairs. The correlation of each performance with g, calculated in this way, is given in Table XV. By means of these and Yule's formula for partial correlations[3], the correlation due to g in the coefficients of Table XIV can be eliminated and the specific correlations determined. These last are given in Table XVI.

(ii) *Correlational groups of tests of similar act but different content.* The most distinct groups of this kind are the sensory discrimination and the sensory memory groups (Tables I and II). Let us consider the first. The vertical and horizontal double lines divide the table into three groups A, B and C. Groups A and B consist of the inter-correlations between the auditory and visual tests respectively, while group C consists of the correlations of the auditory with the visual tests. The average correlations of the three groups A, B and C are ·65, ·20 and ·19 respectively. The average of group A is considerably larger than that of group C, and indicates a fairly large specific auditory factor[1]. This is not the case, however, with the visual group B; and as this might be attributed to the unsatisfactory reliability of the visual tests, the colour and angle tests were repeated in two classes by the second method described on p. 66 of "An improved colour-wheel," this *Journal*, May 1914. However, the correlation was again very low, only ·12. An auditory factor evidently has a wider range than a visual.

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[1] For details of method see "Visual and auditory imagery," this *Journal*, VII. Pt 4, 469.

[2] The proof of this formula is given in "Mental tests of Dementia" by Hart and Spearman, *Journal of Abnormal Psychology*, 1914, 24.

[3] *Introduction to the Theory of Statistics*, 235.

TABLE I. *Sensory Discrimination (inter-correlations).*

		Pitch	Rate	Music	Yellow	Angles	Patterns	Colour groups	
	Pitch	**·70** —	·71 *·035*	·64 *·045*	·04 *·101*	·21 *·070*	–·05 *·091*	·09 *·079*	
A	Rate	— —	**·44** —	·60 *·057*	·42 *·085*	·41 *·072*	·09 *·106*	·04 *·096*	C
	Music	— —	— —	**·57** —	·08 *·128*	·34 *·081*	·38 *·085*	·21 *·093*	
	Yellow	— —	— —	— —	**·52** —	·13 *·108*	·05 *·141*	·33 *·098*	
	Angles	— —	— —	— —	— —	**·44** —	·00 *·107*	·33 *·098*	B
	Patterns	— —	— —	— —	— —	— —	**·36** —	·35 *·098*	
	Colour groups	— —	— —	— —	— —	— —	— —	**·38** —	

Figures in heavy type = reliability coefficients.
Average of total group = ·26 ± *·019*.
Average of group A = ·65 ± *·026*.
" " B = ·20 ± *·044*.
" " C = ·19 ± *·026*.

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[1] The larger value for group A cannot be explained by supposing the auditory performance to depend more than the visual one upon *g*, for Table XV shows the contrary.

TABLE II. *Sensory Memory* (*inter-correlations*).

		Pitch	Rate	Music	Yellow	Angles	Patterns	Colour groups	
	Pitch	**·58** —	·30 *·063*	·55 *·068*	·35 *·088*	·26 *·075*	·30 *·071*	·10 *·071*	
A	Rate	— —	**·57** —	·36 *·075*	·38 *·086*	·65 *·043*	·46 *·062*	·40 *·060*	C
	Music	— —	— —	**·52** —	·27 *·149*	·24 *·084*	·33 *·072*	·41 *·071*	
	Yellow	— —	— —	— —	**·45** —	·41 *·088*	·43 *·095*	·34 *·087*	
	Angles	— —	— —	— —	— —	**·50** —	·60 *·081*	·47 *·055*	
	Patterns	— —	— —	— —	— —	— —	**·69** —	·61 *·048*	B
	Colour groups	— —	— —	— —	— —	— —	— —	**·62** —	

Average of total group $= \cdot 39 \pm \cdot 017$.
Average of group A $= \cdot 42 \pm \cdot 033$.
" " B $= \cdot 48 \pm \cdot 031$.
" " C $= \cdot 35 \pm \cdot 023$.

In Table XIV the results of the visual and auditory discrimination groups of tests have been pooled. The inter-correlations between them and the tactile discrimination group are distinguished by being surrounded by heavy lines. The correlations of Table XIV include both the general and specific factors. In Table XV are the correlations of the three groups with *g*. The highest in the discrimination group of Table XIV is ·26, that between the visual and auditory groups. After the elimination of *g*, however, this is reduced to ·07 (Table XVI). The average correlation between the three kinds of discrimination is, on elimination of *g*, only ·03. The conclusions to be drawn from Tables I, XIV, XV and XVI, with regard to discrimination, are, that there is no such factor as discrimination in general apart from *g*[1]; that a specific factor determining the ability to discriminate only occurs in the present experiments when the same sense is concerned, and is therefore most probably of the nature of content; that as the correlation with *g* only averages ·29, the act of sensory discrimination does not make any very great demand upon the general energy of the brain.

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[1] The statement that there is no such factor as discrimination in general apart from *g* must not be taken to mean that there is no specific discrimination factor, but that there are a number of such factors of very limited range. Nor must it be taken to mean that the discrimination factor is necessarily included in *g*. Our ideas as to the nature of *g* must of necessity be vague until further investigations have been made

Now let us consider the results of the corresponding sensory memory tests. The correlation between the auditory and visual groups is ·44 (Table XIV), as compared with ·26 for the discrimination group. After the elimination of *g* the correlation between the discrimination groups is, as we have seen, reduced to a negligible quantity, but that between the memory groups is ·27 (Table XVI), a significant figure, indicating the existence of a specific factor.

TABLE III. *Verbal Memory and Sensory Discrimination.*

	Pitch	Rate	Music	Yellow	Angles	Patterns	Colour groups
Auditory motor	·08 *·064*	·18 *·076*	·19 *·076*	·38 *·077*	·04 *·075*	·21 *·089*	·12 *·078*
Visual	– ·10 *·064*	·31 *·066*	·00 *·080*	·26 *·084*	·10 *·074*	·09 *·095*	·15 *·079*
Auditory	·03 *·064*	·06 *·073*	·06 *·076*	·30 *·080*	·06 *073*	·02 *·091*	·00 *·079*

Average = ·12 ± *·017.*

TABLE IV. *Verbal Memory and Sensory Memory.*

	Pitch	Rate	Music	Yellow	Angles	Patterns	Colour groups
Auditory motor	·25 *·074*	·23 *·073*	·21 *·082*	·21 *·094*	·17 *·077*	·26 *·073*	·25 *·067*
Visual	·18 *·074*	·05 *·071*	·12 *·082*	·31 *·083*	·09 *·074*	·06 *·078*	·12 *·069*
Auditory	·37 *·062*	·37 *·062*	·23 *·066*	·28 *·088*	·27 *·069*	·21 *·072*	·20 *·067*

Average = ·21 ± *·024.*

Table II shows the correlations between the individual sensory memory tests and is divided into groups similar to those of Table I. The averages for the three groups A, B and C are ·42, ·48 and ·35 respectively. The average for the whole group is ·39 as compared with ·26 for the discrimination group. This, the specific correlation of

Table XVI, and the fact that the averages for the three groups are much more alike than is the case with discrimination, seem to indicate that there is a factor—not very large, it is true—which affects the whole group, that is, that there is a small general memory factor. This has already been noted by both Burt and Abelson. The specific correlations of auditory and visual memory (Table XVI) average ·16. This is a low figure; but no other tests give a higher specific correlation with verbal memory. Hence this also agrees with the existence of a very small general memory factor.

The correlations of the individual tests of the verbal memory group with the individual tests of the sensory discrimination and sensory memory groups were worked out (Tables III and IV); but there does not seem to be any tendency for visual verbal memory to correlate particularly with the visual tests in the other two groups; nor do the auditory verbal tests correlate especially with the other auditory tests, except in the memory group (Table IV) where the correlations of the two auditory verbal tests are distinctly higher with memory of pitch, rate, and musical phrases, than is the case with the visual verbal memory test. The average correlation of Table III is ·12, while that of Table IV is ·21, again indicating the existence of a very small general memory factor.

(iii) *Correlational groups of tests of similar content but different act.* There are two distinct groups of this kind and their correlations are given in Tables V and VI. Table V consists of the correlations between the sensory discrimination and the sensory memory groups of tests. It has been divided into groups similar to those of Tables I and II. In group A are the correlations between the auditory discrimination and the auditory memory tests. In group B are those between the visual discrimination and the visual memory tests. Group C consists of the correlations between the auditory discrimination and the visual memory tests, and group D of those between the auditory memory and the visual discrimination tests. The figures in heavy type are the correlations between corresponding tests in discrimination and memory; *e.g.* the first one is between discrimination of pitch and memory of pitch. In the heavy type correlations only is the content absolutely the same. The following figures are the averages for the four groups:

A = ·34,
B = ·29,
C = ·18,
D = ·29.

TABLE V. *Sensory Discrimination and Sensory Memory.*

		Pitch M	Rate M	Music M	Yellow M	Angles M	Patterns M	Colour groups M	
	Pitch D	**·25** *·067*	·12 *·070*	·63 *·050*	·03 *·093*	·03 *·073*	·00 *·076*	·02 *·069*	
A	Rate D	·16 *·088*	**·38** *·072*	·40 *·052*	·23 *·104*	·08 *·085*	·32 *·080*	·19 *·078*	C
	Music D	·44 *·068*	·23 *·080*	**·57** *·055*	·60 *·082*	·12 *·086*	·21 *·078*	·35 *·070*	
	Yellow D	·27 *·089*	·53 *·068*	·58 *·081*	**·30** *·095*	·31 *·098*	·31 *·101*	·25 *·089*	
	Angles D	–·02 *·082*	·12 *·081*	·20 *·095*	·39 *·094*	**·14** *·085*	·07 *·085*	·15 *·077*	
D	Patterns D	·28 *·091*	·34 *·091*	·31 *·095*	·29 *·141*	·34 *·091*	**·35** *·087*	·41 *·084*	B
	Colour groups D	·06 *·086*	·49 *·068*	·30 *·093*	·33 *·105*	·38 *·084*	·19 *·091*	**·46** *·066*	

Average of total group	=·27±*·012*.
Average of group A	=·34±*·023*.
" " B	=·29±*·024*.
" " C	=·18±*·023*.
" " D	=·29±*·025*.
Average of oblique col. (in heavy type)	= 34±*·025*.
" " aud.	=·38±*·039*.
" vis.	=·31±*·024*.

The average for the whole group is ·27, and the average for the oblique column in heavy type is ·34.

It might reasonably have been expected that the averages of groups A and B would have been higher than those of C and D. But this is true only in the case of group C, and then the difference is only a slight one. The average of the oblique column is surprisingly small; and, as the chief difference between the discrimination and the corresponding memory test was in the time interval between observing and recognising the object presented, the lowness of correlation is mostly due to the dominant influence of this time interval.

The correlations between the pooled discrimination and memory groups in Tables XIV and XVI are enclosed in a square. It will be seen from Table XVI that there is a small correlation between the discrimination and memory tests of the same sense, but practically none when they are different.

In sensory discrimination and memory, therefore, content may be the cause of a specific correlation, but its influence is very much smaller than might be expected *a priori*.

TABLE VI. *Verbal Memory* (*inter-correlations*).

	Aud. motor	Visual	Auditory
Auditory motor	**·70** —	·52 *·049*	·76 *·045*
Visual	— —	**·70** —	·82 *·021*
Auditory	— —	— —	**·69** —

Table VI consists of the correlations between the auditory motor, the visual and the auditory verbal memory tests. In all three tests the content is the same and only the presentation differs. Variation in the mode of presentation might be expected to lead to different methods of procedure in learning, such as the formation of different types of imagery. In such a case it is reasonable to suppose that that presentation which favours the individual's dominant type of imagery will be the most successful. If this theory is true, differences in mode of presentation would be expected to cause considerable reduction in the correlations. This, however, is not the case. Of the three correlations two (·76 and ·82) are high, and the other (·52) is fairly high.

TABLE VII.

Class	Aud. motor verbal memory	Visual verbal memory	Aud. verbal memory
St. II	—	58	47
„ V	—	65	60
„ VII boys ...	61	66	61
„ III	50	57	52
„ V and VI ...	58	64	59

With regard to the amount remembered according to the mode of presentation, Table VII gives the average marks obtained in each of the three verbal memory tests for each of the five classes of children. In every case the result of visual presentation is somewhat superior. Now the correlation of these three tests with visual imagery is practically nil[1], but the correlations between the tests themselves are high. These facts show that although the children under these conditions evidently learn more from visual presentation, it has nothing whatever to do with their ability to visualise.

The results of the experiments of Colvin and Myers[2] also show that children learn more from visual presentation. They themselves, however, interpret their results as indicating that the child, at least until the age of ten, is predominantly a visualiser. But this is an unwarranted conclusion, for, in the first place, in selecting their tests, they assume that memory is dependent upon imagery, that memory of a certain kind of material is indicative of the presence of the corresponding imagery. The only way in which to discover whether these tests actually do determine image type is to correlate the results of all the tests which are intended to determine a particular type, and after eliminating *g* to see if a specific correlation remains. If so, and no other factor can be produced to account for this specific correlation, it would be reasonable to suppose that the factor was imagery. Such correlations cannot be determined from the results of Colvin and Myers as given, for they are in the form of class percentages, whereas the performances of each individual are required. Another test from which they obtained a second series of results consisted of the reproduction of an auditorily presented story, involving three kinds of ideas supposed to call forth visual, auditory and motor images respectively. But many of these ideas might well involve one kind of imagery as much as another. For example—"Then he sprang at Mr Brown" is described as motor, but it might just as well be visual. Henmon also found that superiority with one form of presentation means practically the same degree of superiority with others. He suggests that the closeness of the correlation is due to the fact that with practised adults (as his subjects were) the natural method of learning is the same, no matter what the form of presentation may be. This also appears to be true of children, and if so, matters are considerably simplified. The teacher will not have to study the most suitable mode of presentation for each individual child, but can consider the problem with regard to the class as a whole.

(iv) *Other groups.* In Table VIII are given the correlations between the associated memory, memory of meaning and mechanical memory tests described above. To these also have been added their correlations

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[1] "Visual and auditory imagery," this *Journal*, VII. 484, Table VI.

[2] *Psych. Review, Monogr. Suppl.* II. 1909.

TABLE VIII. *Verbal Memory* (*inter-correlations*).

	Scholastic Intelligence	Auditory motor	Associated memory	Memory of meaning	Mechanical memory
Scholastic Intelligence	**·74** —	·36 *·085*	·20 *·095*	·33 *·090*	·14 *·108*
Auditory motor	— —	**·72** —	·44 *·081*	·32 *·090*	·49 *·076*
Associated memory	— —	— —	**·42** —	·17 *·108*	·44 *·081*
Memory of meaning	— —	— —	— —	**·66** —	·30 *·103*
Mechanical memory	— —	— —	— —	— —	**·70** —

h auditory motor verbal memory and scholastic intelligence. In all
tests the mode of presentation is the same, but the content different.
necessary to note, however, that it does not follow because the
f presentation is the same in all five tests the method of learning
be similar in every case, and for this reason this group was not
th the groups under the second heading of this section. All
tions of this group are lower than those of the other verbal
up (Table VI). The average which is ·36 is much nearer
nsory memory group.

LE IX. *Verbal Memory and School Subjects.*

	[illegible]	Writ.	Arith.	Spell.	Comp.	Need.	Sci.	Hist.	Paint.	Geog.
[illegible]	[illegible]	15 *091*	31 *·075*	·53 *·068*	·20 *·082*	·36 *·087*	·40 *·149*	·35 *·168*	·09 *·077*	·36 *·087*
[illegible]	[illegible]	·19 *·087*	·12 *·080*	·20 *·091*	·17 *·086*	·01 *·096*	−·08 *·174*	−·14 *·182*	·12 *·081*	·16 *·095*
Auditory	·19 *·090*	·24 *·085*	·34 *·074*	·24 *·091*	·35 *·079*	·13 *·097*	·56 *·125*	·54 *·135*	·09 *·076*	·37 *·084*

Average of Auditory motor = ·31 ± *·032.*
„ „ Visual = ·10 ± *·035.*
„ „ Auditory = ·30 ± *·031.*

Table IX consists of the correlations of each of the verbal memory tests of Table VI, with each of the school subjects. The figures given below the table are the average correlations for each horizontal line. It

TABLE X. *Teachers' Estimates and Sensory Discrimination.*

	Pitch	Rate	Music	Yellow	Angles	Patterns	Colour groups
S.I.	·20 *·058*	·32 *·068*	·49 *·056*	·44 *·067*	·14 *·071*	·25 *·083*	– ·05 *·077*
P.I.	·24 *·061*	·30 *·071*	·18 *·074*	·23 *·112*	·26 *·069*	– ·03 *·095*	·04 *·083*
P.T.	·14 *·063*	·00 *·078*	·24 *·073*	·30 *·105*	·20 *·079*	·33 *·086*	·25 *·077*
S.S.	·19 *·060*	– ·08 *·070*	·21 *·073*	·06 *·106*	·20 *·070*	·22 *·087*	– ·03 *·078*

Average of S.I. = ·26 ± *·027.*
„ P.I. = ·17 ± *·031.*
„ P.T. = ·21 ± *·030.*
„ S.S. = ·11 ± *·030.*

TABLE XI. *Teachers' Estimates and Sensory Memory.*

	Pitch	Rate	Music	Yellow	Angles	Patterns	Colou grou
S.I.	·35 *·059*	·31 *·089*	·30 *·062*	·26 *·085*	·23 *·064*	·31 *·066*	[illegible]
P.J.	– ·04 *·096*	·14 *·070*	·19 *·081*	·04 *·095*	·17 *·074*	·09 *·076*	[illegible]
P.T.	·24 *·066*	·32 *·064*	·15 *·081*	·26 *·092*	·22 *·071*	·24 *·07*	[illegible]
S.S.	·26 *·063*	·21 *·064*	·30 *·073*	·26 *·086*	·04 *·071*	[illegible]	[illegible]

Average of S.I. = ·27 ± *·027.*
„ P.I. = ·10 ± *·031.*
„ P.T. = ·24 ± *·027.*
„ S.S. = ·17 ± *·027.*

will be observed that the averages for the auditory tests are in both cases three times that for the visual test. The difference is an interesting one, as much of the teaching in elementary schools is done through the auditory sense. The difference between the visual and auditory tests

TABLE XII. *Teachers' Estimates and Verbal Memory.*

	Auditory motor	Visual	Auditory
S.I.	·31 *·058*	·29 *058*	·44 *·047*
P.I.	·20 *·063*	·24 *·064*	·20 *·064*
P.T.	·35 *·060*	·28 *·060*	·34 *·058*
S.S.	·01 *·064*	·12 *·061*	·18 *·060*

Average of S.I. = ·34 ± *·033*.
„ P.I. = ·21 ± *·037*.
„ P T. = ·32 ± *·034*.
„ S S. = ·10 ± *·036*.

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TABLE *Estimates and School Subjects.*

TAB

Read	Auditory motor	Visual	p.	Need.	Sci.	Hist.	Paint.	Geog.
	·26 *·085*	·21 *·084*	; *38*	·22 *·090*	·77 *·069*	·93 *·168*	·40 *·061*	·63 *·054*
			3 *69*	·43 *·084*	·01 *·180*	·17 *·175*	·38 *·066*	·30 *·091*
			·46 *·060*	·21 *·097*	·69 *·095*	·51 *·146*	·22 *·074*	·48 *·076*
·23 *·067*	·27 *·075*	[illegible] *·085*	·43 *·072*	·33 *·086*	·57 *·119*	·39 *·152*	·16 *·071*	·59 *·065*

	Scholastic subjects	Technical subjects
Average of S.I.	= ·74 ± *·030*	·35 ± *·043*
„ P.I.	= ·32 ± *·044*	·39 ± *·044*
„ P.T.	= ·45 ± *·035*	·27 ± *·047*
„ S.S.	= ·40 ± *·036*	·24 ± *·046*

nical subjects = writing, needlework, painting.
Scholastic subjects = all those not technical.

shows itself particularly in those subjects which are most often taught through the auditory sense, namely, history, science, and geography. Thus the correlations of science with the auditory tests are ·40 and ·56 as against – ·08 with the visual test. Table VII shows that children learn more by visual than by auditory presentation. The correlations

TABLE XIV.

	Schol. int.	Pract. int.	Pains-taking	Social status	Vis. discr.	Aud. discr.	Tactile discr.	Vis. mem.	Aud. mem.	Verbal mem.	Exam. marks	Oppo-sites	Sen-tences	Draw-ings	Hol
Schol. int	**·79** —	64 *·036*	·64 *037*	·56 *·040*	·34 *·058*	·24 *·058*	·14 *·115*	·27 *·059*	·38 *·054*	·38 *·050*	·82 *·027*	·63 *·048*	·49 *·059*	·35 *·073*	·20 *·07*
Pract. int.	— —	**·60** —	·33 *·059*	·19 *·060*	·29 *·067*	·19 *071*	– ·09 *·125*	·15 *·069*	·19 *·066*	·34 *·056*	·59 *·077*	·42 *·054*	·36 *·072*	·20 *·086*	·2
Pains-taking	— —	— —	**·62** —	·60 *040*	·31 *·064*	·15 *·064*	·14 *·123*	·23 *·066*	·26 *·026*	·40 *·053*	·53 *·064*	·36 *·074*	·29 *·076*	·41 *·075*	
Social status	— —	— —	— —	**·72** —	·20 *067*	·28 *·057*	·05 *·119*	·05 *·065*	·26 *·061*	·10 *·060*	·18 *·082*	·28 *074*	·18 *·078*	·24 *·0*	
Vis. discr.	— —	— —	— —	— —	**·53** —	·26 *·068*	·00 *·131*	·45 *·061*	·38 *·064*	·22 *·066*	·32 *·084*	·51 *·067*	·17 *·088*		
Aud discr.	— —	— —	— —	— —	— —	**·72** —	·01 *·119*	·15 *·066*	·37 *·057*	– ·01 *·061*	·23 *·082*	·46 *·065*	·27		
Tactile discr	— —	— —	— —	— —	— —	— —	**·57** —	·09 *·123*	·11 *·122*	– ·11 *·114*	·09 *·174*	– ·05 *·180*	—		
Vis. mem.	— —	— —	— —	— —	— —	— —	— —	**·64** —	·44 *·054*	·28 *·061*	·18 *·085*	·42 *·071*			
Aud. mem.	— —	— —	— —	— —	— —	— —	— —	— —	**·68** —	·33 *·058*	·09 *·086*				
Verbal mem	— —	— —	— —	— —	— —	— —	— —	— —	— —	**·81** —	·25 *·078*				
Exam. marks	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —					
Oppo-sites	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —					
Sen-tences	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —			—	*·078*	·1 *·08*
Draw-ings	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	**·72** —	·51 *·06*
Holes	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	**·7** —

of science, history and geography with auditory and visual verbal memory also show that the mode of presentation has a distinct influence. It is probable, then, that if these three subjects were taught more through the visual sense, there would be a general increase all round in the amount remembered.

In Tables X, XI, XII and XIII are given the correlations of the teachers' estimates with the sensory discrimination, sensory memory, verbal memory and school subjects groups respectively. The average correlation of each estimate is given immediately below each table. The averages for the school subjects are all higher than the corresponding averages in any other group. This is to be expected in the cases of scholastic intelligence and painstaking, as the teachers drawing up the lists would judge mainly according to school work. In all the tables the averages for painstaking are low. This is interesting to note, as painstaking is often cited as one of the general functions of the mind. However, its influence is evidently more limited than is often supposed. The figures of Table XVI show that in many cases after the elimination of *g* its influence is practically nil as compared with actual ability in any performance.

TABLE XV. *Correlations with the General Factor.*

Scholastic Intelligence	·75
Practical Intelligence	·52
Painstaking	·54
Social status	·37
Visual discrimination	·50
Auditory discrimination	·39
Tactile discrimination	·00
Visual memory	·47
Auditory memory	·52
Verbal memory	·37
Examination marks	·59
Opposites	·71
Sentences	·59
Drawings	·42
Holes	·22

The correlations between the teachers' estimates themselves are given at the beginning of Tables XIV and XVI. Practical intelligence was expected to give a low and even negative correlation with social status, on account of the fact that children of lower social status are less cared for, and left more to their own resources, than those of the higher classes. The actual correlation between them (Table XIV) is ·19, low, but not negative. There are, however, specific correlations (Table XVI) of both practical intelligence and social status with

TABLE XVI. *Specific Correlations.*

	Schol. int.	Pract. int.	Pains-taking	Social status	Vis. discr.	Aud. discr.	Tactile discr.	Vis. mem.	Aud. mem.	Verbal mem.	Exam. marks	Oppo-sites	Sen-tences	Draw-ings	Holes
Schol. int.	— —	·43 *·042*	·47 *·038*	·46 *·037*	–·06 *·052*	–·05 *·052*	·14 *·093*	–·14 *·053*	–·01 *·054*	·16 *·051*	·72 *·037*	·22 *·067*	·09 *·068*	·07 *·091*	·15 *·071*
Pract. int.	— —	— —	·07 *·072*	·00 *·071*	·04 *·053*	·00 *·053*	–·09 *·094*	–·12 *·053*	·11 *·053*	19 *·050*	·41 *·063*	·07 *·069*	·07 *·069*	–·02 *·072*	·14 *·070*
Pains-taking	— —	— —	— —	·51 *·036*	·05 *·053*	–·07 *·053*	·14 *·093*	–·03 *·053*	–·03 *·053*	·25 *·051*	·31 *·068*	–·04 *069*	·03 *·069*	·23 *·068*	·11 *·069*
Social status	— —	— —	— —	— —	·02 *·053*	·16 *·053*	·05 *·094*	–·15 *·053*	·09 *·053*	–·05 *·053*	–·05 *·075*	·03 *·069*	–·03 *·069*	·11 *·070*	·26 *·068*
Vis. discr.	— —	— —	— —	— —	— —	·07 *·055*	·00 *·056*	·29 *·054*	·16 *·055*	·05 *·053*	·04 *·075*	·25 *·065*	–·17 *·067*	·1[illegible] [illegible]9	·03 *·072*
Aud. discr.	— —	— —	— —	— —	— —	— —	·01 *·055*	·00 *·055*	·20 *·053*	–·17 *·052*	·00 *·075*	·28 *·064*	·05 *·068*	·04 *·071*	·20 *·068*
Tactile discr.	— —	— —	— —	— —	— —	— —	— —	·09 *·094*	·11 *·092*	–·11 *·092*	·09 *·094*	–·05 *·095*	–·04 *·095*	— —	— —
Vis. mem.	— —	— —	— —	— —	— —	— —	— —	— —	·27 *·048*	·13 *·052*	–·14 *·073*	·14 *·068*	·04 *·069*	·06 *·071*	·09 *·071*
Aud. mem.	— —	— —	— —	— —	— —	— —	— —	— —	— —	·19 *·052*	–·32 *·068*	·15 *·068*	·13 *·067*	·01 *·071*	–·07 *·071*
Verbal mem.	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	·04 *·075*	·07 *·069*	·16 *·068*	·00 *·071*	–·05 *·071*
Exam. marks	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	·05 *·075*	·41 *·063*	·40 *·063*	–·04 *·075*
Oppo-sites	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	·50 *·052*	·11 *·070*	·03 *·072*
Sen-tences	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	–·01 *·071*	·03 *·072*
Draw-ings	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	·47 *·055*
Holes	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —

scholastic intelligence, probably due to the teachers being under the suggestion of the examination results, when drawing up the lists. If this is eliminated by means of Yule's formula for partial correlations, the correlation between practical intelligence and social status becomes – ·26.

The specific correlation between scholastic intelligence and painstaking is one which might be expected; that between scholastic intelligence and social status is perhaps partly due to the parents of the better class children taking more interest in their studies. The highest specific correlation in this group is ·51, that between painstaking and social status. As, however, all the children were from the lower working class, one is only justified in inferring that within these limits, social status is dependent upon power of application.

The high specific correlation between scholastic intelligence and the examination marks is one to be expected, as every teacher or head-teacher drawing up lists would be considerably influenced by the examination results. The average correlation of the teachers' estimates of scholastic ability with the scholastic subjects (Table XIII) is ·74 and with the technical subjects, writing, drawing and needlework, ·35. This considerable difference between the two averages justifies the distinction already made between these two groups of subjects.

(v) *Conclusions.* In the process of discrimination in the several senses, the present experiments show there is no common factor of the nature of act. Any specific factor which may occur is of the nature of content, and this is especially so with regard to the auditory sense. Hence there is no such function as discrimination in general apart from *g*.

With regard to memory, there appears to be a very small general memory factor. Content also may cause a small specific correlation, but the range of content as a specific factor is not so pronounced in auditory memory as in auditory discrimination. There is no specific factor, however, between simple sensory memory and the more complex memory in the same sense, *e.g.* between memory of pitch or rate and auditory verbal memory.

Between simple acts of memory and discrimination, a specific correlation exists when the sense concerned is the same, but is small even when the content is identical.

In memory of verbal material a difference in the mode of presentation reduces the correlation less than a difference of content. A difference of presentation, however, does not necessarily imply different act, whereas it is not unreasonable to suppose that a difference in a complex content might call forth a different act.

A process like painstaking, which by some has been regarded as a general function of the mind, is much more limited in its influence than is generally supposed.

On the whole the results support a view similar to the earlier one of Thorndike, if the general factor is admitted, and the results of Dr Sleight. Specific factors are probably numerous; they can be of the nature of act or content. All except the motor factor are very limited in range. The other specific factor of the school subjects described as "the association between written words and their meanings" is probably a group of simpler specific factors.

From the present experiments it seems probable that a visual mode of presentation is more efficacious in teaching children than an auditory mode; but further investigation is necessary before any definite conclusions can be drawn.

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THE FORMATION OF PROJECTED VISUAL IMAGES BY INTERMITTENT RETINAL STIMULATION.

BY GEORGE H. MILES.

II. APPARATUS, PROCEDURE, AND RESULTS.

I. *Introduction.*
II. *Description of apparatus.*
III. *Account of experiments.*
IV. *Suggested explanations of phenomena observed.*
V. *Colour phenomena in the projected image.*
VI. *Colour changes observed in the projected image.*
VII. *Retention of the projected image and connection with immediate visual memory.*
VIII. *Summary and conclusions.*

I. INTRODUCTION.

IN a previous paper an account was given of the peculiarities observed when the image of a small bright light, such as a glowing incandescent filament, briefly fixated, is revived by intermittent stimulation. In these experiments the stimulation was produced in a number of different ways—by simply blinking the eyes, by moving the hand rapidly between the screen and the eyes, viewing the screen through the sectors of a rapidly revolving disc, intermittent illumination of a semi-transparent screen or by the intermittent illumination of a screen placed in a dark room. Comparing the results obtained by these methods, it was found that the best effects were noticed when the light was completely excluded from the eyes between each stimulus. In addition, it was noticed that there was a certain favourable rate of stimulation necessary to produce the brightest image; and, further, that this rate was more rapid for small bright images than for larger images. In order to investigate these differences more thoroughly, a special form of apparatus was planned, which might allow of considerable changes in the rate of stimulation. By its means also variations in the ratio of stimulus-time to rest-period could be readily adjusted, and measured with a fair degree of accuracy. In the present paper a description of the apparatus and an account of its use in a preliminary personal survey of the conditions for producing the clearest images will be given. Following this, an account of tests applied to a number of individuals will indicate the scope of the method and emphasise the wide range of individual variations. A close relationship also appears to exist between the factors involved in the duration of the image, and those associated with visual memory. A short account of experiments which led to this conclusion will be noted.

II. DESCRIPTION OF APPARATUS.

It was decided in the first place that easily controlled shutters which would completely shut out all light from the eyes were necessary and experiments were made to determine the best form. A pair of revolving discs was first tried, but in order to control the ratio of stimulus to rest periods, too great complications were introduced and further, at low speeds, the eyes followed the movement of the discs, and the formation of a steady image seemed impossible. A vertically moving slit, operated by electromagnets in connection with a special regulating commutator, was next tried, but again the same distraction of eye movement at low speeds arose while at high speeds the moment of inertia of the moving parts led to inaccurate timing so that although the commutator might be set to a ratio of 3 : 4, for duration of rest to stimulus, the shutter would take up a different rate. Finally a form was adopted in which the opening was from the centre, across the eyes, in a manner somewhat similar to the opening of the eyelids. Much better results were obtained. The moving parts were so designed that the moment of inertia was practically negligible and accurate response to the commutator was ensured. The speed of the commutator and, consequently, the rate of the shutters could be easily altered by a disc drive, and further the ratio of exposure to rest could be altered at will without the subject's knowledge. In addition one shutter could be set to open at any desired interval before or after the other. It was also possible to close completely either shutter at any time during an experiment.

The essential parts consist of shutters for each eye arranged so that the following changes are under the control of the operator:

(*a*) Frequency of stimulus,

(*b*) Duration of stimulus,

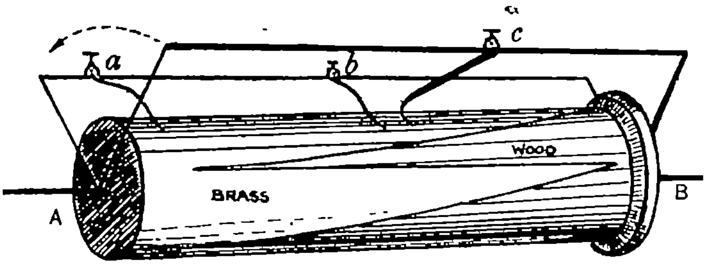
(*c*) Coincidence of stimulus, *i.e.* stimulation of one eye might be simultaneous, advanced or retarded, with reference to the other eye.

Each shutter is worked by electromagnets which are in connection with and under the control of a commutator.

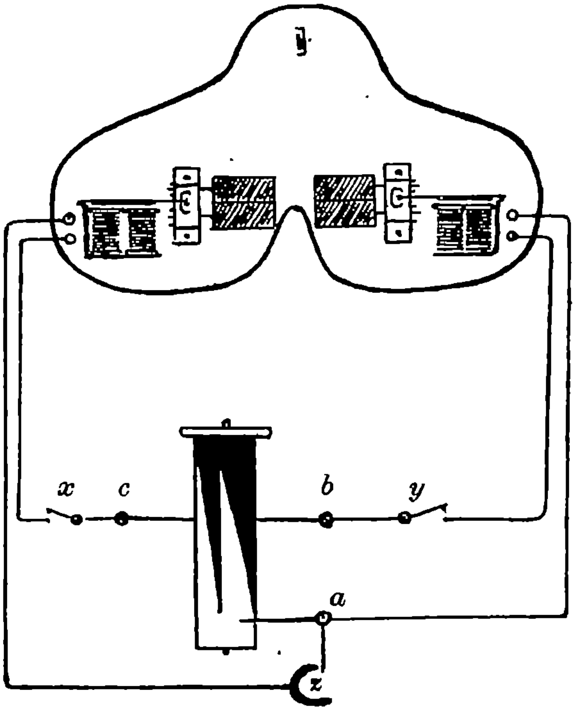
The commutator is constructed in the following manner. A cylinder 12 cm. long and 25 mm. diameter has a brass tube of the form shown (Fig. 1) inlaid in it. The brass is continuous at the base but is divided into four parts 1 cm. higher up, and each part then tapers to a point at the end.

The curved triangles so formed are right-angled, so that in rotation the advancing front of the triangle is parallel with the axis of the cylinder. The end (*A*) has a fixed contact (*a*), and two other contacts (*b*) and (*c*) are arranged so that they can be easily moved in a direction parallel with the axis of the cylinder. Contact (*b*) is in circuit with he electromagnet which controls the shutters of the left eye and tact (*c*) is in circuit with the shutter for the right eye. Both are ected with the common contact (*a*) through the battery. When r contact (*b*) or (*c*) is touched by the advancing edge of one of the ular brass strips of the commutator, the circuit is closed, and [illegible] tter closes in response to the electromagnet excitation and remains closed so long as the contact is on the brass strip. As the brass strip tapers in width the duration of the closure can be altered by moving the contact towards the end (*B*) of the commutator where the brass tapers to a point. If moved nearer the base (*A*), the closure of the circuit will occupy a greater part of the commutator's revolution and the shutter will consequently be closed for a longer time. In the same way the time which the shutter remains open can be varied. By altering the rate of rotation of the commutator the stimuli can be made to follow rapidly or slowly while the position of the contacts (*b*) and (*c*) will determine the ratio which the stimulus-period bears to the rest-period. In addition, contact (*c*) can be adjusted so that its circuit is completed either in advance of or after that of contact (*b*). In this way the moment of stimulation of one eye may occur a fraction of a second before or after that of the other. The duration of the stimulus may be made the same for both eyes, or it may be greatly varied according to the distance of contact (*b*) from contact (*c*).

The rate at which these stimuli and rest periods occur can be altered from, say, one per second to fifty or more times per second; the upper



I. Commutator.



II. Connexions between commutator and shutters.
x, switch for right-eye circuit.
y, ,, left-eye ,,

Fig. 1

limit being reached when the 'lag' of the magnet and the inertia of the moving parts of the shutter are too great for the construction of the magnet and the battery power at command. For variations at a higher rate than this a different form of apparatus would be required; but so far, all indications seem to show that no optical changes would be noticed at a higher rate. The variations in rate of stimulus are obtained in this apparatus by working the commutator with a disc friction drive; a movement of the commutator friction wheel from the centre of the disc to the periphery resulting in an increased rate of movement. The disc is driven and regulated by a gramophone motor. The shutter is constructed of two narrow light strips of thin card, revolving as shown (Fig. 2) about their longer diameter by attachment to spindles.

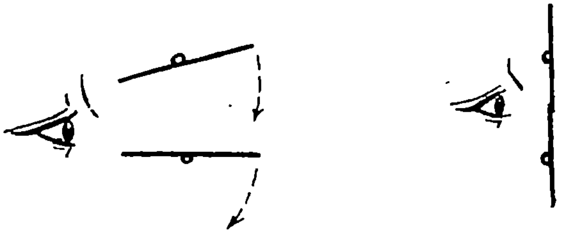
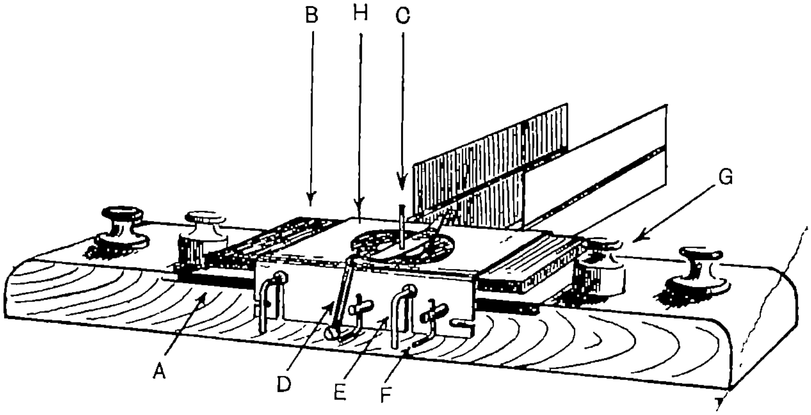

Open Closed

Fig. 2.

The spindles are rotated by rolling on a strip of glass (*A*, Fig. 3). In this way, a roll equivalent to a quarter of the circumference of the spindle will ensure closure. The friction and inertia being practically nil, a rapid and accurate response to the electromagnet is obtained. The rolling of the spindles is brought about by a light strip of hard wood (*B*) which presses on the rollers, and is worked backwards and forwards by the armature of the magnet, acting on the pin (*C*). Sufficient pressure to prevent slip of the rollers is obtained by two thin rubber bands (*D*) which are passed over the ends of a transverse bar attached to the upper wood strip and under two pins which project from the sides of the wood base. The rollers are kept in alignment by rolling freely in slots cut in the sides of a light brass cover (*H*) which goes over the wood strip. The upper central part of this cover is removed so that the bar carrying the rubber bands, and also the upright pin (*C*) over which the slot of the armature-arm works, can move freely.

To counteract the effect which even an infinitesimal slip of the rollers over the surface of the glass plate might have in summation, stops (*F*) are provided, which ensure that the shutters are in the same position at each opening and closing.

Adjustable hard rubber stops (*G*) are placed at the end of the travel of the upper wood slip, and the brass cover is slotted at the sides so that it is kept from falling off when the apparatus is placed in a vertical position. In addition to this, further adjustment is provided so that the minimum travel of the electromagnet armature gives the maximum that is required for the efficient movement of the shutters. The electromagnet armatures are carried on springs so that prompt recovery follows stoppage of the current, and in addition, the rubber bands (*D*) that hold the upper moving wood slip against the roller are attached in such a way that they assist in rapid recovery. By these means, and by the small travel that is required of the armature, the length of time that the shutter is closed is the same as the duration of the current from the commutator.



Details of mechanism for opening and closing of shutters—
- *A.* Glass plate.
- *B.* Hard wood slip.
- *C.* Pin attached to wood slip and worked by electromagnet arm.
- *D.* Rubber pressure bands.
- *E.* End of shutter spindle.
- *F.* Checks for shutter spindle.
- *G.* Check to limit travel of wood slip.
- *H.* Sliding brass cover, slotted to keep spindles aligned.

Fig. 3.

Both shutters are mounted in front of openings in a board which is shaped so as to fit over the nose and can be held in place across the forehead (Fig. 4). A raised rim is placed round the shutters so that when they are closed no light can come to the eyes. The openings behind are covered with transparent gelatine so that there is no air current from the rapidly moving shutters to affect the eyes and a shaped rubber rim, covered with black velvet, is arranged so that all light from behind the board is shut out.

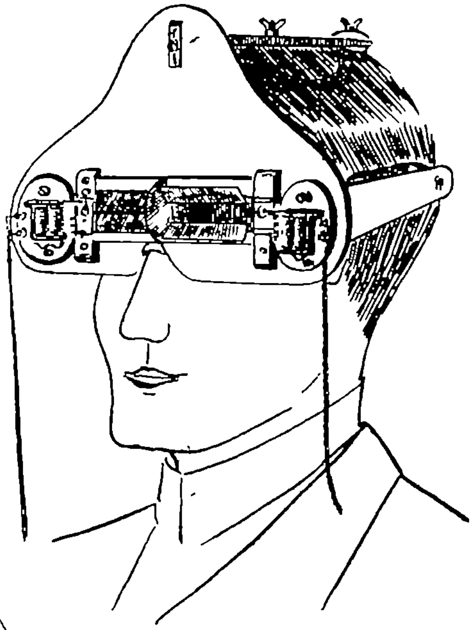


Fig. 4. Showing shutters and head attachment.

A padded bar with an adjustable balance weight passes over the head and two padded side-arms keep the apparatus securely in position. This method of attachment allows of great ease of movement. The image can be projected in any direction and on any surface to which the eyes are directed. At the same time the hands are free to manipulate the commutator and changes are easily made. In this way it was possible to make, first, a rapid survey, and afterwards more exact observations of the most obvious phenomena.

III. ACCOUNT OF EXPERIMENTS.

An account of a few typical experiments will give an idea of the method of using the apparatus.

The bare light of an incandescent mantle was fixated for two seconds at a distance of 150 cm. Then the shutters were set working at a slow speed and the character of the image was noted. On turning to a brightly lighted surface, the stimulus appeared intermittent and the image also fluctuated considerably, but when the screen was less brightly illuminated, the image was steadier and the flicker of the shutters was not noticeable. The commutator wheel was now drawn slowly across the revolving disc and thus the speed of the shutters was increased till, at a stimulation rate of eight per second, with a duration of the flash stimulus of about 54σ, the image was very steady, and remained so until it disappeared. A new exposure was made and the shutters were set working at the rate of eight per second, with a stimulus of 54σ and a rest or closure period of 71σ; and the changes were noted. The image was at first very bright and white and then gradually changed to yellow with a border of light green. This spread over the surface till the image was entirely green and finally disappeared. On reducing the intensity of illumination of the screen, the image reappeared, still of a green colour, gradually becoming fainter till it disappeared. The rate of stimulus was now reduced to four times per second and the image again appeared of the same colour. A third time it was brought back, by increasing the period of rest till the ratio of stimulus to rest was about 1 : 4.

A number of experiments finally determined a rate of eight per second as the best, and a ratio of stimulus to rest was finally fixed at $54\sigma : 71\sigma$ as giving the best average result. The screen was illuminated with an ordinary inverted Welsbach incandescent mantle placed at a distance of about 70 centimetres.

In order to give a more uniform ground the mantle was placed within the focus of a lens and this lens was covered with a diaphragm having a disc of two centimetres diameter cut in it. Viewed at a distance of 120 cm. this gave a bright uniform disc, giving practically foveal vision. Similar results were noted.

In order to test a larger area of the retina, the apparatus was fixed on the head and the eyes were directed for two seconds to the cross bars of the window frame, 100 cm. wide, at a distance of about 250 cm.

The shutters were set at the previous rate, and on looking at the ceiling an image followed, but of only about 40 secs. duration. Again an exposure was made, the catch or window fastening being fixated. The rate of the shutters was reduced step by step till after repeated experiments a rate of four per second was found to be the best. The commutator contacts were now brought towards the base so that the rest period was increased and a great improvement was found. Finally the best results were obtained when the stimulus period was to the rest period as 1:5, and stimulations occurred at the rate of four per second.

With a uniform, white, cloud-covered sky, an exposure of two seconds now gave a stimulus image lasting for three minutes. . Equally good results were obtained if after the exposure intermittent light from the window was allowed to fall through the shutters upon the closed eyelids. The results were sometimes so vivid, that it was with difficulty that one believed that the eyes were really closed, and it was only on opening them and noting that the direction of view was quite different, that one could be convinced that light was not entering by partly closed lids.

An idea of the great sensitiveness of the method is seen in the following tests. The cross bars of the window were fixated for fifteen seconds, and although the light was so poor that 35 minutes were required to darken the sensitive paper of an Imperial exposure-meter to the standard tint, the image obtained by this short exposure lasted for 11 minutes, though of course towards the end of the time the image had become duller. For at least eight minutes the image was clear, changing from positive to negative, the lines being sharply defined. The image gradually disappeared from the centre outwards, though at times, when it had apparently disappeared, it reappeared again with quite its original intensity with the exception of the part at the immediate centre. This experiment was carried out with a very dimly lighted screen and with shutters moving at the rate of four per second.

The early disappearance of the centre of the image when the stimulus light is only weak can best be seen when the light from a long narrow slit is allowed to fall on the retina, the middle point of the slit being steadily fixated. With a rate of four stimulations per second a clear white or black image can be seen stretching across the whole field. The centre of this line gradually disappears and at the same time the line becomes distinctly yellow in colour.

At times during some of the experiments the whole condition of consciousness so changed that all semblance of flicker, either in image or shutter movement, disappeared, and it seemed as if one were looking out of the actual window in the grey twilight of early morning. No colours appeared when both shutters worked in unison. On advancing the commutator contacts, so that one eye was stimulated 45σ later than the other, the window-frame image and distant chimneys appeared of an intense black on a deep red ground and the impression was given that the sky was of a dark ruby colour.

The succession observed in the changes of the image was first, black crossbars on a white ground, then gradually this changed to a suspicion of pink, then through very rapid stages to a deep red ground, followed a little later by a change to black on white, and finally after one or two fluctuations, positive and negative, the image gradually faded as a negative.

The stimulus period for the formation of the large projected image appears to be about the same as that for the small central image, that is in the neighbourhood of 40σ to 55σ for the same intensity of stimulus-screen illumination. The principal difference appears to lie in the rest period. For images due to the central retinal illumination the best results were obtained with a rest period of about 70σ, whereas for images resultant on rod stimulation the rest period was as long as 200σ.

Numerous experiments were carried out with the apparatus, using lights of widely differing intensities and under most diverse conditions. From the mass of experience gained, a number of facts stand out clearly and give an indication of the factors involved in the production of the image. A summary of the results of a number of tests showed in my own case that, with the shutter-rate and screen-intensity constant, the clearest and most prolonged images were formed under the following conditions:

1. Bright light stimulating a small retinal area:

Rate of stimulus	8 per second
Duration of stimulus	54σ
Duration of rest	71σ

2. Duller light, *e.g.* cloud-covered sky, stimulating large retinal area:

Rate of stimulus	4 per second
Duration of stimulus	50σ
Duration of rest	200σ

It was evident that these rates and the intensity of the screen illumination represented only a mean value. At the commencement the rate was too slow and the intensity of the screen too low to give the best results; whereas towards the end the image could always be prolonged by decreasing the rate and lowering the intensity of the illumination of the screen. Under ideal conditions the decrease in the rapidity of the stimulation and the intensity of the screen illumination would take place at such a rate as would keep pace with the gradual recovery of the originally stimulated tract. These facts seem to show that there is an intimate connection between the adaptation of the whole retina and the recovering adaptation of the previously excited area, when the best results are obtained.

When the stimulus-rate and the intensity of the illumination are kept constant the image changes uniformly from positive to negative. No reversals whatever are noticed if the object fixated be bright and the area small. When larger areas were fixated, such as say the window framework, reversals took place. They were more marked at slow rates and when the shutters gave a short stimulus period followed by a long rest period. Strange fluctuations of attention were also noticed. At one time the image formed by the short exposure seemed to predominate in consciousness whereas at another period the image formed during the longer rest period occupies entire attention. Under these conditions the ordinary after-image effects seem to prevail though with a marked increase in intensity and clearness. In the case of bright central exposures a distinct play of colours was noticed. This was more marked with longer and brighter exposures, though in spite of all precautions no marked constancy of colour changes could be noted other than that, with short exposures, white-grey was constant; for longer exposures, in the majority of cases, the final colour was green. The vividness of the colours varied from time to time—some being exceedingly brilliant and at other times they were with difficulty distinguished. There appeared to be no discoverable connection between these changes and fatigue of eye or body or the state of health.

Numerous experiments in connection with images formed after fixation of lights of fair intensity were carried out with the object of discovering the method of formation of the image. The following results are typical:

In one experiment a bright light was fixated for about ten seconds. Attention was then directed in succession to a number of coloured discs about 3 cm. in diameter which were pasted in various positions on a white screen. Immediately on fixation of a disc so that the projected image of the bright light completely covered it, the disc disappeared and the screen in that part appeared uniformly white. Similar discs placed on coloured screens disappeared on fixation and in their place was the induced colour of the screen. A blue disc was pasted on a red sheet of paper; and over this a yellow sheet was placed. In the centre of this was a hole through which the blue disc could be seen, and this sheet was cut in half through the hole so that it could be removed by drawing aside the two halves, thus exposing the under sheet. A bright light was fixated and the image thrown on the blue disc, which appeared in the centre of the yellow sheet. Immediately the disc disappeared and the field of vision was a uniform yellow. On rapidly withdrawing the yellow sheet, the red sheet underneath appeared quite uniform and only on looking to another part of the sheet could the blue disc be seen by indirect vision. Blix[1] has given an account of homochromatic induction of this type which takes place under ordinary circumstances, in the case of small white or black areas placed on a brightly coloured ground and fixated for a period. This has been ascribed to dispersion but on previously fatiguing an area by exposure to bright light it was possible to obtain immediate induction over a disc 12 cm. in diameter. The 'spread' occurred immediately the disc was brought within the area that had been previously stimulated. Dispersion could hardly account for the change over so large an area and the alternative seems to be to assume a spread or irradiation of impulses from neighbouring tracts. Finally, white and coloured discs placed on a black velvet screen and viewed after the fixation of a bright light were completely obliterated and the screen appeared as a uniform black surface.

If the shutters are set working when a white disc has been obliterated in this way, and fixation is maintained, the disc gradually reappears, showing at first faintly. The colour of the background slowly disappears and on complete recovery of the tract the disc becomes white. In time the colour gradually spreads faintly over the disc; and then suddenly the disc disappears completely for a few seconds, reappearing again tinged with the inducing colour. With coloured discs the phenomena are more obscure, being complicated by the play of colours which are sometimes present in the fading after-image.

When the white disc on the black ground is similarly treated, a uniform black ground is seen at first; and then the disc gradually appears as if seen through a smoked film. At no time is a positive image seen, yet, on turning the eyes to a white screen at the earlier stages of recovery, a bright positive image immediately appears. This immediately turns to negative on again fixating the white disc placed on the black ground.

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[1] *Skandin. Arch. f. Physiol.* v. 13.

These experiments seem to indicate that the tract initially excited at first refuses passage to the impulses set up by the light from the discs. They also seem to show that the block has occurred at levels lower than those at which consciousness is affected. The fact that the whole field is uniform indicates either the presence of psychical factors which prevent the consciousness of a lack of sensation at the stimulated area, as is usually stated to be the case with the blind spot, or there is a uniform spread or irradiation of impulses from surrounding parts at levels between the block and the cortex.

IV. SUGGESTED EXPLANATIONS OF PHENOMENA OBSERVED.

A physiological explanation of these changes is difficult. It is evident that owing to the initial stimulus some change takes place in the excited tracts that at first entirely prevents, and later allows only part of the impulses to pass through. Evidently, in any explanation, some account of the mechanism of blocking is necessary. At the synapses there is a break in physiological continuity. But though there is no continuity of processes, there is close contiguity where the body of one cell is surrounded by the branchings of the axon, or where the dendrons and axons of a number of cells ramify. It has been established that the nerve fibres themselves are practically unfatiguable, and there is a wealth of evidence that seems to show that the seat of resistance to the passage of impulses lies in the synapses. The blocking of impulses has been spoken of as due to the accumulation of fatigue products but the negative character of the evidence[1] of chemical or thermal change during activity is against this theory, and the clear outline which is maintained by the projected image throughout its course seems to point to the absence of any products which, by diffusion previous to subsequent absorption, would affect surrounding tissues and give rise to an indefinite image. The blocking effect seems to be confined to the actual paths that have been previously opened through the synapses. Such a block or increase of resistance would be produced by a retraction of the cell processes and, though it is improbable that this movement will ever be seen actually taking place, there is abundant indirect evidence in support of the possibility of such action.

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[1] Gotch, 'Nerve,' Schafer's *Text-book of Physiology*, 454.

A few instances of observations which serve to illustrate the grounds for this theory will also assist in the subsequent application of the hypothesis to the facts noted in connection with the projected image. Wiedersheim[1] has reported having observed slow changes of form in the cells of lower organisms. Hodge[2] noticed changes in the nuclei of nerve cells which in the resting condition were vesicular and round, whereas after long continued stimulation the nuclei of similar cells were shrunken and of irregular contour. Mann[3] examined the cells of the retina and the occipital lobe and found that enlargement took place at first, but in cases of prolonged stimulation the nucleus and protoplasm were considerably reduced in bulk. It seems hardly probable that changes of such magnitude as are to be observed in this way would take place without a corresponding change in the finer processes.

The action of drugs has also been observed to alter the appearance of the processes. Verworn[4] has compared the results that he has obtained in the case of Rhizopoda narcosis with those recorded by Demoor in the case of narcotised ganglion cells. The filaments of the ganglion cell appear varicose, are retracted and show a remarkable likeness to the changes in the filose pseudopodia. Similarity of appearance, and action such as this, suggests the possibility that the nerve cell may react under much weaker stimuli, if of a suitable nature, in a manner similar to that of protoplasmic bodies in general. That the extension of the nerve cell can be carried out under the influence of incredibly delicate directive stimuli is seen in the whole development of the nervous system as the animal matures and in the phenomena relating to the regeneration of fibres. In these cases, however, the extension is carried out very slowly. Exceedingly minute stimuli can, if of suitable nature, exert an immediate directive influence over protoplasmic activity. This is seen in the case of chemotaxis and other allied phenomena. As an example of this, the action of the fern spermatozoid is interesting. Pfeffer[5] found that with a 0·001 per cent. solution of malic acid, diffused through a drop of water, there was sufficient difference of concentration at the poles of a spermatozoid 0·015 mm. in length to produce immediate directive action,—a chemical difference far too slight to be detected by any physical means at present known. Similarly the slight differences in electrical potential produced at the poles of minute organisms, when a weak current passes through the liquid in which they are immersed, produces a definite directive action. In the light of innumerable facts of a similar nature and in view of the changes which invariably take place in simple cell forms under suitable stimuli, it seems impossible to deny movement to the extremely fine processes of the highly organised and delicate nerve cell, when subjected to the stimuli which are peculiar to the nervous system. The character of the nerve impulse is obscure. The evidence of chemical changes, even in the grey matter, is a subject of controversy; but the existence of electrical changes is easily demonstrated. Even the slight electrical variations produced when light falls on the retina will cause instant extension or retraction of the mercury filament of a capillary electrometer. It seems to be far more in accordance with observed facts to assume that there is a corresponding movement at the synapses during these changes in electrical condition, than to conceive the chain of cells as playing the part of merely passive conductors of the impulses.

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[1] Wiedersheim, *Anat. Anz. Jena*, v. 673.

[2] Hodge, *Journal of Morphol.* VII. 95.

[3] Mann, *Journal of Anat. and Physiol.* XXIX. 100.

[4] Verworn, *General Physiology* (Eng. trans.), 377.

[5] Quoted by Verworn, *loc. cit.*

Anatomical considerations further show that the number of fibres in
e optic nerve is less than in the 'optic radiation' and is also less than
number of retinal end elements. In addition the existence of
ipetal fibres has been demonstrated. Hence it is not surprising
change in a part of the visual system, under certain circumstances,
he whole. Electromotive changes have been observed in the occipital lobe of the rabbit when the retina was stimulated by light[1]. Englemann[2] has described a retraction of the cones of the retina under the influence of light, and has further noted a similar action in the cones of the unstimulated eye. A close and delicate connection is seen in the electrical variations which occur in the two eyes after uniocular stimulation has taken place. It is conceivable that these electrical changes, or the factors underlying their production, may bring about alterations in synaptic proximation which would lead to the binocular contrast effects that have been observed. Similarly produced proximation and retraction might also account for the facts noticed during distraction experiments where a slight stimulus, through a different channel, frequently leads to enhanced attention, whereas more intense stimuli lead to a lowering of attentional activity. As a further

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[1] Beck, *Centralbl. f. Phys.* IV. 16.

[2] *Arch f. d. ges. Phys.* XXXV. 498.

example, a faded after-image[1] produced in one eye can b ived by weak light stimuli passed through the lid of the unaffec . The stimulus can be looked upon as causing throughout the system a protrusion of processes which have become retracte to the absence of sufficiently powerful stimuli. When this e the weak stimuli due to the continued activity of cer f the tract are now able to bridge over the lessened synap and affect consciousness. A similar action in the same the case of liminal stimuli. A just perceptible sma nore distinctly if other small parts of the same re bly stimulated[2]. Conversely a strong stimulus, su to retraction, applied to a part of the retina woul ion throughout the system; and this leads to a diminution of the intensity of the impulses aroused by the stimuli from areas less brightly illuminated, and so gives rise to contrast effects. With intense stimulation there would be marked sympathetic retraction of the paths immediately surrounding the stimulated tracts. This probably accounts for the thin black line frequently noticed as surrounding the bright projected image.

It seems as if under certain circumstances a rhythmical eff is produced in the passage of the impulses across the syna It has been shown that the passage of impulses of conside rapidity, centrally given, are reduced, probably at the sy to a uniform rate of ten or twelve per second. The r image[3], which appears on momentary stimulation of the retina, appears to be due to the intermittent passage of impulses through the synapses such as might be brought about by rhythmical approximation of processes under the influence of continued retinal activity. In short, a consideration of (1) the histological evidence as to changes in form of the nerve cells under stimuli and drugs, (2) the activity under appropriate stimuli of similar types of cells, and (3) an analysis of the peculiarities noticed in conduction and effects of nerve impulses, leads to the conclusion that it is highly probable that the nerve cell processes are capable of protrusion and retraction under suitable stimulation.

From the above and numerous similar observations it seems reasonable to hypothecate two types of reaction at the synapses.

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[1] McDougall, *Mind*, IV. 217.

[2] Fick, *Arch. f. d. ges. Phys.* XLIII. 445.

[3] McDougall, this *Journal*, January 1904, 112.

In the first case there is a marked and prolonged retraction of the processes, occurring in consequence of the general exhaustion and shrinkage of the cell. In the other case there seems to be the possibility of extremely sensitive immediate responses to differences in magnitude of stimuli of normal intensity and duration. There also appears to be, between related elements of the system, a sympathetic connection, which, during changes in intensity of stimulation, probably leads to changes in contiguity of cell processes throughout the system. This constitutes adaptation to the intensity of the stimulus.

The most probable sequence of changes seems to be the following. When a cell is at rest, there will be a tendency to assume as compact a form as possible owing to the influence of surface tension. This will tend to a retraction of processes. At the same time, in this condition, the slightest stimulus will be effective in causing an extension of processes. When this takes place, the processes of neighbouring cells are brought into close proximity if not into direct contact, leading to a passage of the impulse to the next link in the chain, which in turn passes it on till at last cortical levels are reached. It has been shown
hat the intensity of the sensation produced by a given stimulus falls
rapidly as the duration of the stimulus is prolonged till at last the
mal level is reached[1]. The rise to maximum intensity can be looked
as due to the proximation of the processes, the high intensity
ng caused by the reduction of synaptic resistance due to the
contiguity, and the final waning of intensity to normal levels
due to a corresponding retraction of the processes.

ntense or prolonged stimulation it has been observed on
examination that the cells have become reduced in size.
tion would naturally cause a retraction of the processes,
increasing resistance to the passage of impulses. This
eduction in the intensity of the sensation such as can
prolonged fixation of a bright light. Where changes
taken place, the chain of cells will for some time
weaker stimuli owing to the increased synaptic
the retraction. The greatest blocking will take
es which lie at the peripheral end of the chain,
synapses of the inner and outer molecular layers of
, leaving, unless the exposure has been very intense or
nged, the higher synapses only slightly affected.

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[1] McDougall, this *Journal*, June 1904, 180.

When the whole retina is subsequently subjected to a succession of momentary stimuli, impulses of high intensity will pass through the whole system with the exception of that part of the previously stimulated channels where blocking has taken place. If the initial stimulus was of only short duration, but little change would have taken place in the cells higher than the first synapses. There is thus ample opportunity for the spread of impulses at each of the synapses between the affected cells and the cortical levels at which neural action is associated with changes in consciousness. As a consequence the image following on a short bright exposure is exceedingly bright. If on the other hand prolonged stimulation has resulted in extensive blocking or retraction of processes, the passages offered to the spread of impulses are few, and the image is in consequence of less intensity at first, and may be entirely absent.

As stimulation is continued there is an increasing brightness of the image though as yet no direct impulses from the retina can pass owing to the block which still exists at lower levels. At last, with further recovery, a direct path is formed, the possibility of spread from neighbouring areas is reduced and direct impulses can pass from the retin
to cortex again. These are however very feeble in comparison w
those from the rest of the retina. At this stage the image app
'black' or grey and the fixation point on the screen becomes vi
As further recovery takes place the strength of the direct im
approaches that of those from the rest of the retina, and th
fades till with complete recovery the whole screen become
in character.

V. COLOUR PHENOMENA IN THE PROJECTED I

The projected image produced in the way prev
is so readily observed even by unpractised subjects
excellent means of testing individual peculiarit
account gives an indication of the method ado

In the preliminary experiments with the ap
vidual differences were noticed, both in the len
image lasted and in the after flight of colours. In
more information concerning these differences, a number
were tested under similar conditions. Previous experiments
indicated that variations in the intensity and duration of the stimulus-light, and also differences in the illumination of the screen on which

the image was projected, had a considerable influence on the character of the image. In testing a number of subjects it was essential that these conditions should be kept constant. The following arrangements were made to ensure this.

A light box was constructed so that the observer could be completely shut in, and a screen of white paper was uniformly illuminated at a distance of 56 cm. from the subject. In the middle of this screen a circular opening 7 cm. in diameter was cut, and behind this was a ground-glass plate. On the side nearest the observer a black cross was placed so that it showed symmetrically behind the opening. In front of this was a shutter that was operated by hand. When closed, a fixation point on the paper-covered surface of the shutter occupied the centre of the opening in the screen. Behind the ground-glass plate was a pendulum shutter which gave a regular exposure of two seconds. An inverted incandescent mantle was used as a source of light; and a condenser focused light from this on the ground-glass screen. The board holding the eye shutters previously described was suspended in the light box in such a way that on looking through the openings the fixation point on the screen, the centre of the cross on the ground-glass screen and the source of light were in complete alignment. Throughout exposure and during projection the screen was uniformly illuminated by a screened incandescent light, so that all work was carried out with the bright-adapted eye. The observer sat on a stool the height of which was adjustable; and after allowing time for adaptation to the light from the screen was told to look at the fixation point on the hand-controlled shutter. The signal 'ready' was given and the hand shutter was opened. Immediately the pendulum shutter gave an exposure of two seconds. The subject had been previously warned to maintain fixation of the centre of the cross on the ground-glass screen, and then immediately after the shutter closed he fixated a point 10 cm. below the aperture. Directly the screen shutter closed, the current was passed through the commutator, and the shutters in front of the subject's eyes began to work, giving eight stimulations per second. Short and definite instructions as to the manner of reporting the observations had been previously given to the subject. The duration of the image, the extent and character of the changes, and any peculiarities in appearance were recorded by two assistants as the observer rapidly described the phenomena. The subjects tested were pupils of from 14 to 15 years of age attending a Secondary School.

General observations.

In my own case the exposure of two seconds gave, with the intensity of the light used, only a colourless image of about ten seconds duration. With most of the children however the image showed a considerably longer life, and the reports of colour changes frequently came with bewildering rapidity.

With some subjects the rapid flicker of the shutters, long continued, seemed to produce considerable general fatigue. In the case of three subjects, after each test their closed eyelids twitched at practically the same rate as that at which the shutters had been working. When the eyes were open there was a clearly noticeable but less pronounced flicker of the eyelids. After the image had ceased to be visible on the brightly illuminated screen seen through the moving shutters, these subjects reported that for some time after the tests they could still see a projected image of the disc on the dark brown walls of the room. When the eyelids had regained the normal condition, these subjects were again given an exposure of the same duration, but were unable to see any image on the wall, showing that the arousal of the image had been assisted in the previous cases by slight variations in the light acting on the retina, caused by the twitching of the eyelids. The sensitiveness of the eyelids does not seem to be related to factors which determine the duration of the after-image but seems to be concerned with the general health of the individual. The flicker of the lids is probably due to the persistence of a muscular contraction initiated by the reflexes induced by the bright light entering the eyes at regular intervals.

Most of the subjects reported that the disc was uniformly coloured and readily described the changes that took place. In two cases, however, the subjects reported a series of rings of colour and gave the predominant colour. Colour changes were also reported as taking place near the fixation point of the screen, when the image had completely disappeared. One subject reported changes in colour of the whole screen whenever the image disappeared. In one experiment the projected image persisted for 127 secs., and was followed 13 secs. later by a blue screen which lasted for five seconds and then turned white, after which at 147 secs. the projected image reappeared for six seconds. The screen then became alternately blue and white. The image again reappeared and finally faded, leaving the whole screen blue. Then the colours yellow, green and blue came in succession from the outside to the centre, and at times the whole screen became quite black. These changes appear to be of the same order as those noted by Fechner[1] and Bidwell[2].

All the boys but two saw the image clearly from the first exposure, but in the case of the girls, ten required a second exposure. The colour changes also did not seem so well marked in the case of some of the girls; and, further, a number reported that the image disappeared and reappeared a number of times before final extinction. Only two of the boys reported similar fluctuations. As the girls were tested at a different time from the boys, these differences gave rise to the suspicion that external conditions were responsible. The apparatus was most carefully tested several times; and in addition my assistant was tested. His record both for duration of the image and for the colour changes was practically the same as that obtained when the boys were tested. Apart from these differences, the results obtained with the girls confirm those previously obtained from the boys.

VI. COLOUR CHANGES OBSERVED IN THE PROJECTED IMAGE.

During most of the tests a complete record of the colour changes was kept. The subjects had all received considerable training in colour work in the Art room under the same master, and to ensure that they named the colours correctly they were asked to write down the names of the colours in the colour ring printed in Dr Myers' *Introduction to Experimental Psychology*[3]. No instance occurred which showed inability to name these colours sufficiently well to give reliable results. The subjects were told to report, first, the appearance of the fixation-cross image, and then immediately after to state the colour of the disc upon which it appeared. Immediately the subject announced a change, an assistant called out the time of the change to the nearest second, which time, together with the subject's observations, was recorded by another assistant. A glance at these records emphasises the fact that, in visual experiences, individual differences are most marked. A closer examination of the records of the colour appearances shows that in a few cases the colour changes in the projected image are very scanty. In these cases, too, the image is of short duration. In fact it becomes clear from these results that what is light of considerable intensity for some subjects, and results in a very vivid play of colours in the projected image, is for others of such feeble power that it gives an image of short duration and produces only a limited series of changes. This

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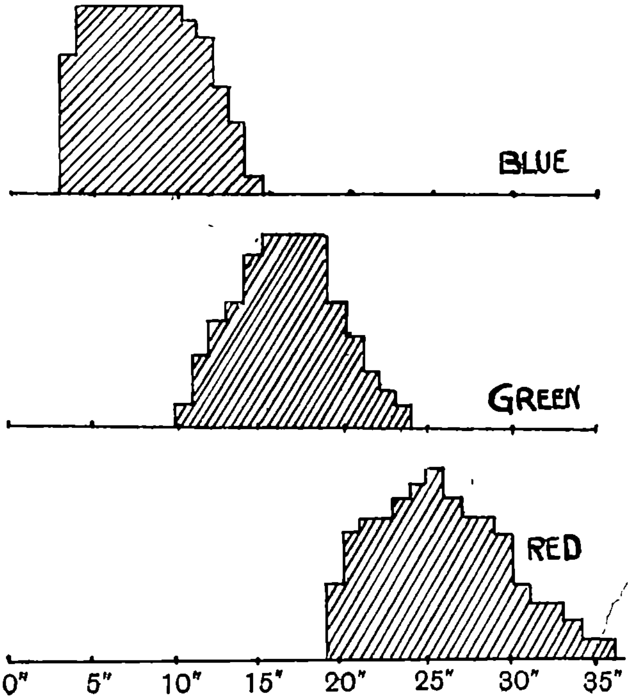
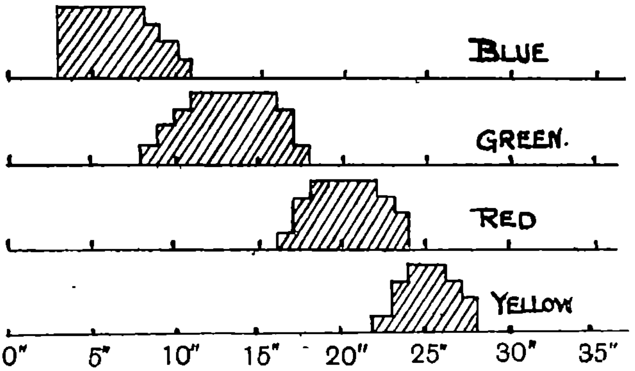
[1] Fechner, *Ann. d. Phys. u. Chem.* XLV. 227.

[2] Bidwell, *Proc. Roy. Soc.* XI. 368.

[3] Myers, *Introduction to Exp. Psychology*, Plate II.



(*a*) With fixation-cross



(*b*) Without fixation-cross

Fig. 5 (Subject J).

seems to show that a regular colour sequence for light of any particular physical intensity is dependent on personal characteristics and is not a resultant of the absolute intensity of the light. The presence or absence of complementary colouring in the central cross of the image, or what would have been the halo if the stimulating disc had been viewed against a dark ground, also appears to be due to some inherent

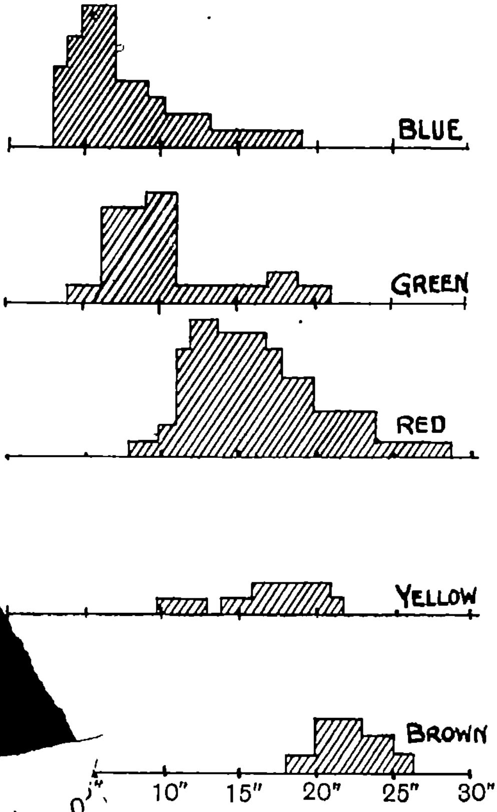


Fig. 6 (Subject C).

our apparatus of the individual. This is seen in a stimulus of the same intensity some subjects, who id colouring of the disc, only saw the image of the either black or white whereas others report colours are in the main complementary.

Subject (S) reported the image of the cross as appearing black every time after 17 exposures of two seconds duration. With exposures gradually increased to 18 seconds, he reported the cross as being black 125 times and 21 times as yellow. The disc-changes most frequently followed the sequence blue, green, red, with yellow occurring irregularly between each of these colours.

In order to analyse these results the following method was adopted. From a subject's record of the colour changes an analysis was made of the number of times that a certain colour, say red, was observed at each second after the initial exposure. Using equal divisions along the X axis to represent successive seconds after the original exposure, and

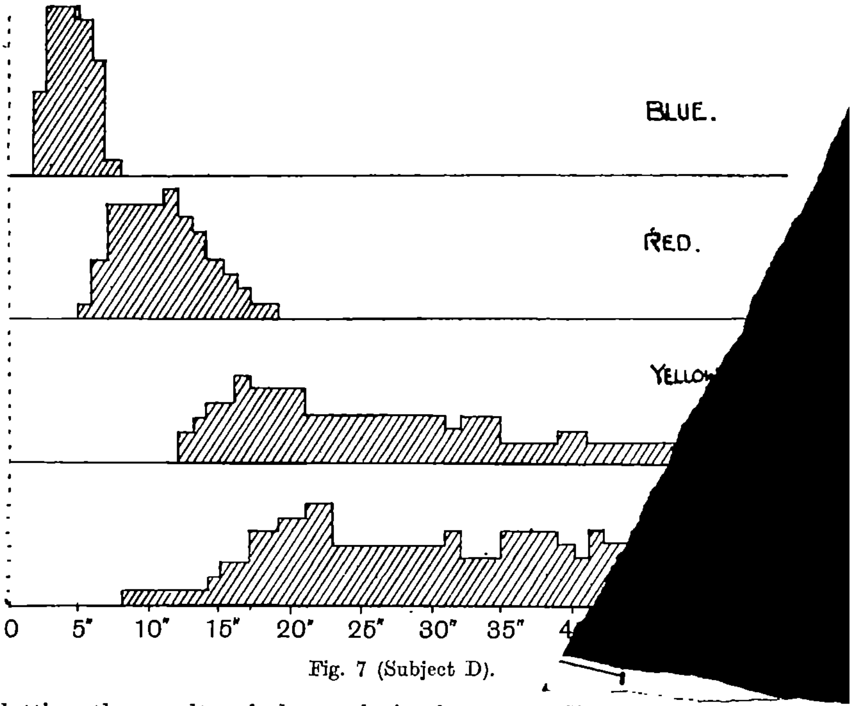


Fig. 7 (Subject D).

plotting the results of the analysis, frequency diagrams of the type shown were obtained. Each colour observed was dealt with in a similar way and the results of the tests gave distinct evidence of a regular sequence of colour changes in many subjects. That of subject (J) (Fig. 5) is particularly regular. The sequence blue, green, red occurs in all the tests, and the frequency diagram illustrates this. This subject was also tested with a plain disc in which the fixation cross was absent and the results showed a similar sequence. Subject (C) (Fig. 6) shows a less stable condition. There are in the records several instances of reversal of sequence; but the frequency diagram shows the predominating order. In the case of subject (D) (Fig. 7), the first two changes appear to be invariable, but as the image fades there seems to be a complex struggle, yellow appearing as frequently as green. In these three subjects the fixation cross image appears either black or white. Subject (A) (Fig. 8) frequently reports the fixation cross as complementary coloured to the disc, but here again there is evidence of inconstancy both in this respect and in the colour sequence of the image of the disc. The duration of the image from test to test shows a steady decrease. An analysis of the records in the same way as noted above brings out the complementary relationship of the colours of the disc to those of the cross in this case much more clearly than the separate records.

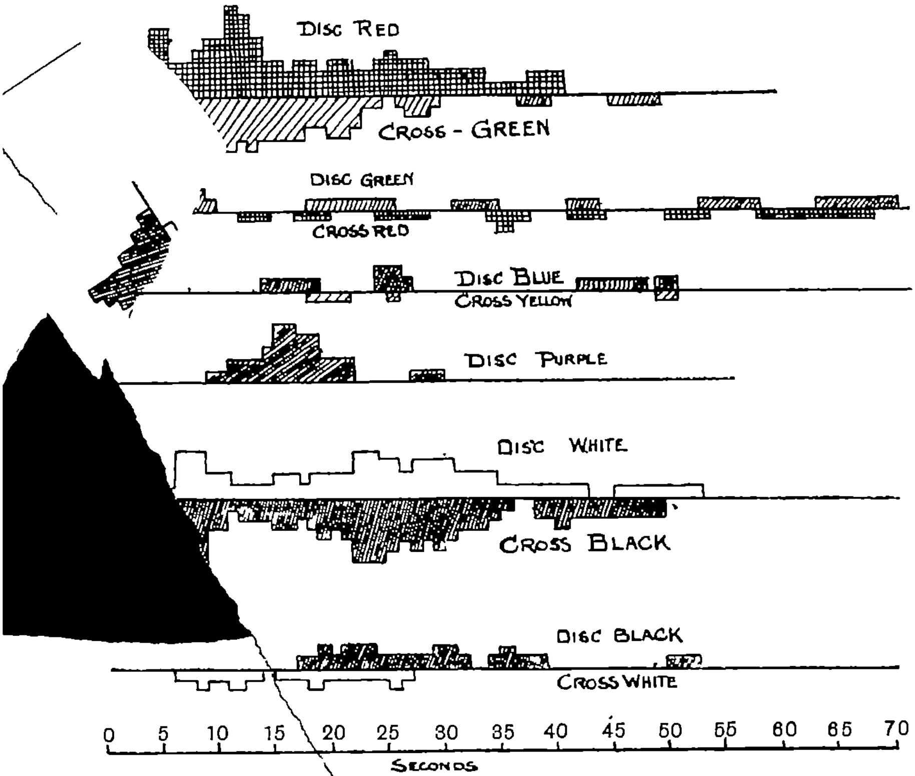
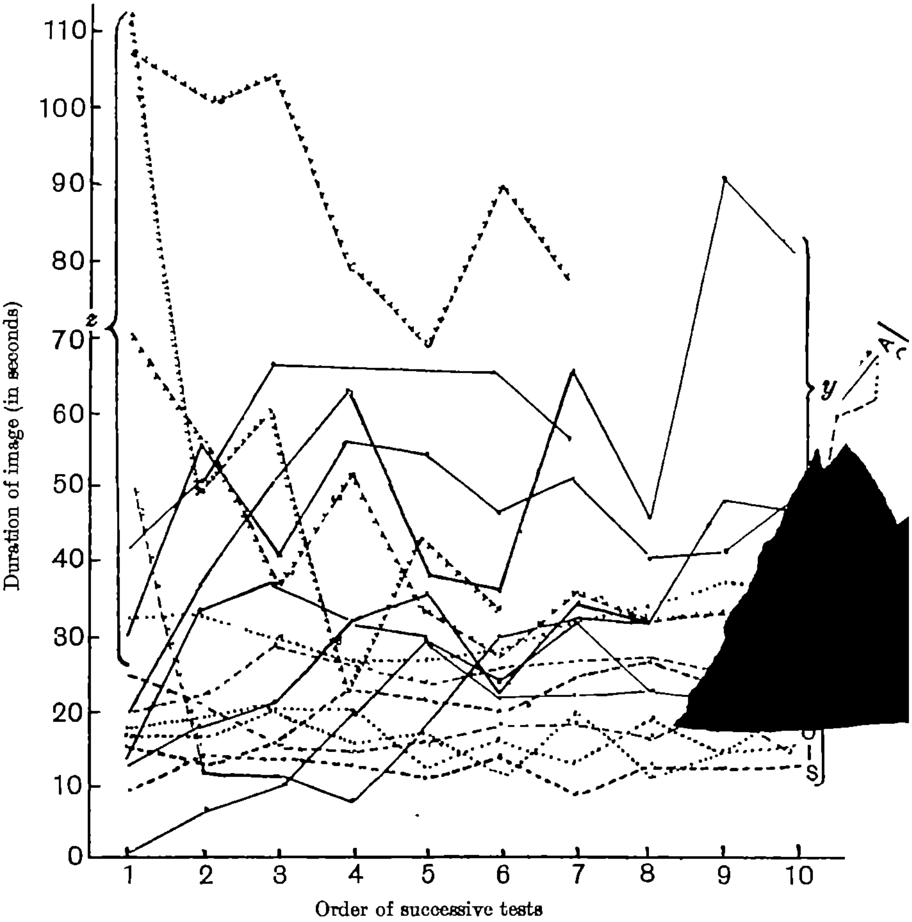


Fig. 8 (Subject A).

The above examples are typical of a number of the records. Some, however, show but little evidence of any predominating sequence of colours; and, though quite a large number of tests were given to one of these subjects, no definite order prevailed.

An examination of the records for the duration of the image brings out several interesting features. At the outset considerable differences



x. Group showing fairly constant duration with successive tests.
y. ,, ,, increasing ,, ,, ,,
z. ,, ,, decreasing ,, ,, ,,

Fig. 9.

were noted in the way that the subjects responded to the successive tests. Some appeared to show very little variation in the duration of the image from test to test (*x*, Fig. 9). Other subjects began with a fairly long duration, and with each succeeding test the length of time that the image was retained diminished (*z*, Fig. 9). A third class showed the reverse effect (*y*, Fig. 9). In an extreme case no image was seen after the first two exposures. Then followed, at successive tests, images which lasted

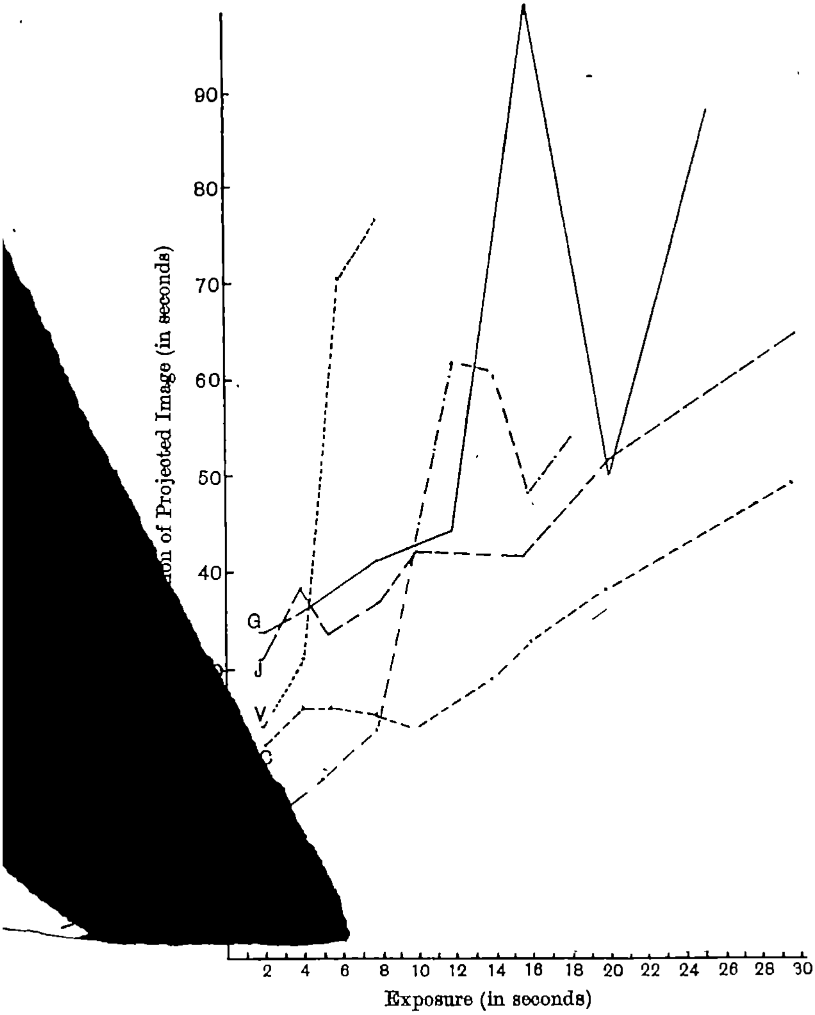


Fig. 10.

n these two latter types the effects of fatigue ay an important part. The results have been r of typical cases and are shown in Fig. 9.

Certain subjects who were more easily available were tested for exposures of longer duration. Here again they were given a period of rest after each test. The length of exposure was regularly increased and the duration of the projected image was noted. On plotting the average duration of the image against the length of exposure, the characteristic relation of these two factors could be examined. The results obtained for five subjects are noted in Fig. 10. The exposures range from two seconds to 30 seconds. It is evident that here again there are distinct individual differences. Subjects (J) and (C) seem to show fairly steady increases. Subjects (S) and (G) show a great increase with longer exposures but later their records become very erratic, due most probably to fatigue. Subject (V)'s tests were discontinued owing to eye strain.

VII. RETENTION OF THE PROJECTED IMAGE AND CONNECTION WITH IMMEDIATE VISUAL MEMORY.

One of the main objects of these tests was to find out if any connection existed between the degree of retention of the image and th [illegible] subject's ability in certain tests for immediate visual memory. T [illegible] reasons for anticipating some such connection are given below. [illegible]

In all work on the immediate memory there is the possibility [illegible] continued activity or sensibility of the previously excited tracts [illegible] recall. In tachistoscopic experiments dealing with the span of a [illegible] it has been noticed that the conditions of adaptation, and the [illegible] of the post- and pre-exposure fields, play an important [illegible] recall. Wundt[1] expressly states that in these tests per [illegible] images are to be avoided, and Pillsbury[2] considers tha [illegible] "mental after-image" are attended to successively a [illegible] of this image determines the number of objects th [illegible] Hylan's[3] work confirms this view. If this be co [illegible] span of attention is really a measure of the persist [illegible] image.

It is extremely difficult to separate the [illegible] distinguish at all between a faint projected i [illegible] image during these tests. The faintest sensations are sometimes sufficient to give rise to definite images if only they resemble those produced by the things which the image represents. James[4] mentions the fact that frequently words, visions, etc., unconnected with the main stream of thought will flit through the mind, and are easily perceptible to an attention on the watch for them. He attributes their appearance to the influence of undetected after-images and states: "I have not a few times been surprised after noting some idea to find, on shutting the eyes, an after-image left on the retina by some bright or dark object recently looked at, which had evidently suggested the idea. 'Evidently,' I say, because the general size, shape and position of the object thought of, and of the after-image, were the same although the idea had details which the image lacked. We shall probably never know just what part retinal after-images play in determining our thoughts. Judging by my own experience, I should suspect it of not being insignificant."

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[1] *Zur Kritik tachistoskopischer Versuch.*

[2] Pillsbury, *Attention*, 68.

[3] J. P. Hylan, "The Distribution of Attention," *Psych. Rev.* 1903, 373.

[4] James. *Principles of Psychology*, II. 88.

Common observation shows how frequently, when an effort of memory is made, the eyelids are blinked, closed, or the eyes are directed to the wall or ceiling. This does not seem to be due entirely to the desire to escape from the stimulus of surrounding objects. Such actions lead to rapid changes in the amount of light entering the eyes and are avourable to the formation of projected images. It is possible that e above individual peculiarities are of service in assisting the memory roducing a slight sensory stimulus which is sufficient to give a clue, serve as a foundation for a more accurate recall.

the light of these considerations it seemed more than likely that rison of the duration of the projected image with the individual's nce in visual recall tests might, in the case of a number of ndicate some connection between the two abilities.

f the so-called visual memory tests are complicated by such a number of additional factors that it is doubtful what part the visual impression plays in the recall. In the tests carried out with the subjects of the previously noted experiments, an attempt was made to eliminate the use of mnemonics and verbal symbols so that, as far as possible, visual factors predominated in the recall.

A brief description of the method adopted is necessary in order to give some idea of the character of the tests.

Letters, figures, geometrical figures, colours, and nonsense syllables are all open to the objection that they are easily named and in consequence the memory of them may be carried, in part, at least, in verbal terms. In order to eliminate this possibility, complexes of lines having no resemblance to any known object were used. The use of material of this type however raises difficulties in the testing of the subject's accuracy of recall. If the subjects be asked to draw what they remember, too great an advantage is gained by those who possess ability in this direction and individual differences wholly unconnected with memory are introduced. Then again, if the subjects are tested by their ability to pick out the previously exposed figure from amongst a number of others, recognition plays too prominent a part. A subject might readily recognise the previously exposed object and yet have but small ability in recalling its appearance. In addition contrast effects, due to the presence of a number of similar objects, add further complications.

In order to solve the difficulty of testing the accuracy, and to render the work of recording the recall as simple as possible, the following method was adopted in principle.

A figure composed of four definite parts each consisting of complexes of lines was used. Each subject was provided with a scoring sheet, having outlined on it four blank spaces in positions corresponding to the four parts of the figure. The whole figure was shown, and then after a few seconds a duplicate of one of its parts was exposed. The subjects then placed the number 1 in that blank on the scorin sheet which they believed corresponded to the position occupied [illegible] the exposed part in the previously shown whole figure. Duplicat [illegible] the remaining parts of the whole figure were then exposed success [illegible] and the subjects placed the numbers 2, 3 and 4 in what they thou [illegible] be the appropriate spaces on the scoring sheet. In this way the [illegible] figure was, so to speak, built up from memory of the positio [illegible] part figures. The subjects' records were easily checked, and t [illegible] of recording presented practically equal facility for all s [illegible] so eliminated differences due to this factor.

When a subject, in this way, indicated correctly the positions which the parts occupied in the whole figure, he showed that he could recall more or less definitely the originally presented card. The parts composing the whole figures were complex in character, and from the reports given by the subjects at the end of a series of exposures, only a few instances occurred where the subject found that he had remembered by verbal symbols.

Several cards of a similar type were prepared and exposed. Each card was exposed for two seconds and then after an interval of four seconds the component parts were each in turn shown for two seconds. Two seconds were allowed for recording after each showing.

The degree of accuracy with which a subject built up the card from the successive exposures of the parts was taken as a gauge of the ability to recall the object and was easily determined by the total of the subject's correct placings throughout the series of tests.

As previously stated, there was considerable difference in the way the subjects reacted during the tests for the duration of the projected image; some showing a fairly constant duration in the successive tests, others showing an increase in the time of retention, probably due to

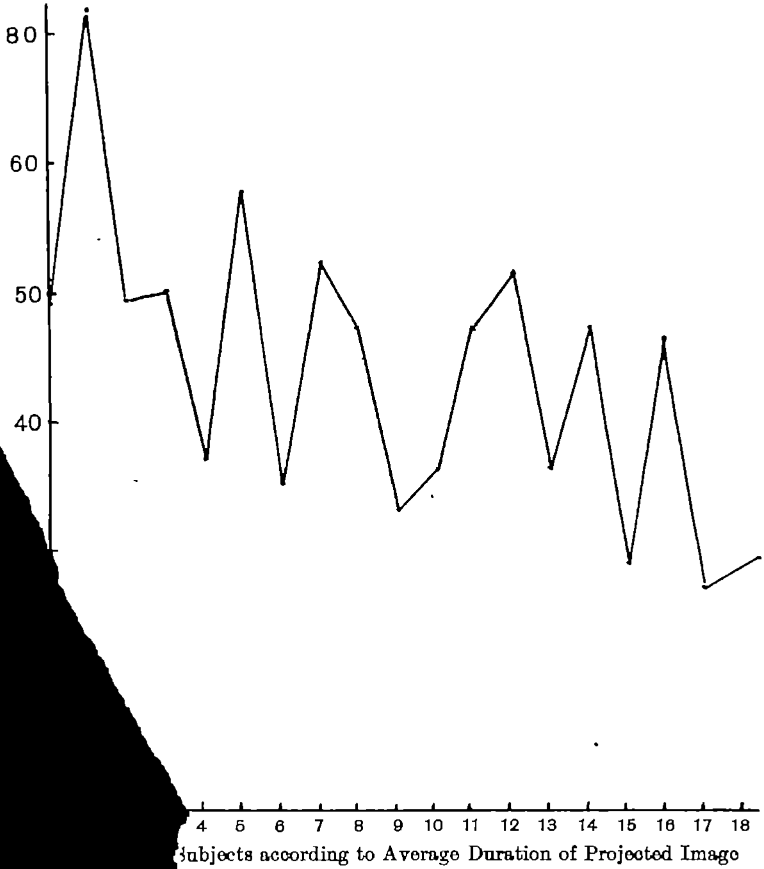

Fig. 11.

rs showed a tendency to diminish. In order that ld be similar to those operative in the memory te the effects of practice as far as possible, the he images consequent on the first five exposures as taken as a gauge of the subject's retentivity litions. The subjects were arranged in order of retentivity, and a graph was obtained by plotting against each subject his score for the visual memory tests. The results are shown for boys in Fig. 11. Using the formula $\frac{6 \cdot \Sigma (d^2)}{n(n^2-1)}$, the correlation for 22 boys works out at 0·57 with a P.E. of ·104, while for the 19 girls the result is 0·653. These results seem to support the suggestion previously advanced that in the case of immediate visual memory a more or less adequate image, perhaps sometimes of a merely fragmentary character, is revived, and though not necessarily distinctly present to consciousness, nevertheless exercises a determining influence over subsequent acts by which the recall is announced by the subject.

VIII. SUMMARY AND CONCLUSIONS.

In these papers an endeavour has been made to present a number of experiments in connection with vision which seem to indicate the following conclusions:

There is, first of all, a need for a distinction between the after-image proper, and the projected image.

1. The after-image depends for its sensory characteristics mai[illegible] upon impulses aroused by continued activity in some portion of [illegible] originally stimulated parts.

2. The projected image appears to be mainly dependent [illegible] formation upon the following conditions:

(*a*) Changes which occur in the intensity of the light re[illegible] the whole, or upon a considerable part of the retina.

(*b*) Altered conductivity of that portion of the visual [illegible] was originally excited.

3. The changes in the intensity of the stimulatio[illegible] retina probably give rise to a series of impulses of consi[illegible] throughout the whole tract. The differences in con[illegible] by the original stimulation, in certain portions [illegible] cause a differentiation of the impulses aroused by t[illegible] and give rise to the sensory characteristics of the [illegible]

4. The positive image is formed when some [illegible] peripheral, of the originally traversed tract is resistant to the [illegible] of the momentary impulses whereas the remaining portion facilitates the passage of impulses which reach it through lateral paths.

5. The negative image, on the other hand, appears, when, owing to recovery of the previously excited tract, direct impulses are able to pass from retina to cortex and lateral spread is thus cut out. These

impulses, by reason of the greater resistance which the recovering paths still offer, are of less intensity than those from other parts. Hence the image produced appears negative.

6. The conjunction of volitional activity with the passage of these impulses through cortical paths leads to the projection of the image. (Part I, sect. v.)

7. The direction of the projection seems to be intimately connected with factors concerned in the voluntary movement of the eyes and is related to some visual schema rather than to the actual direction of the visual axis. (Part I, sect. v.)

8. Many of the characteristics of the projected image suggest an nalogy, which may prove to be a direct connection with the phenomena bserved in certain types of hallucinations. (Part I, sect. IV.)

An explanation of the progressive blocking at the synapses of the ve tract, such as many of the experiments suggest, and the arousal eries of impulses by the intermittent stimulation of the retina seems to be included in the wider conception that the nerve cells respond in a peculiar manner to suitable stimuli with which they are functionally ssociated. The response takes the form of a retraction or protrusion processes, probably infinitesimal, but still of sufficient magnitude to e a material increase or decrease in the synaptic resistance. This ption is based on the following grounds which, though individually conclusive, taken together seem to warrant a provisional sis of the type suggested:

Physiological evidence shows that nerve cells, under the action ged stimulation, show signs of shrinkage and considerable structure.

ction or protrusion of processes under suitable stimuli have been observed in protoplasmic cell-forms of a lower and less highly organised type.

(*c*) A large number of facts in connection with visual phenomena seems to indicate the occurrence of such action within the nervous system.

In the light of these considerations the following assumptions have been made:

(1) There is an immediate protrusion of processes on the sudden mission of light to the eye, or on a change in the intensity of the mination. This change probably accompanies the electrical variation noticed under these conditions, occurs throughout the cell community of the visual tract, and facilitates the passage of impulses which produce

a considerable momentary increase in the intensity of the sensation aroused.

(2) Following this, there is an equally rapid withdrawal of processes to the normal position suitable to the state of adaptation for the average stimulus-value of the total light entering the eye. This leads to an immediate fall to sensation of normal intensity.

(3) With continued stimulation of the same tracts there is a gradual change in the body of the cell, which leads to further retraction, and if prolonged causes complete blocking at the synapses.

(4) This blocking is greatest at the peripheral synapses, thus protecting the higher levels, which are only affected materially when the system has been under prolonged and intense stimulation.

The disappearance of the projected image on the cessation of the intermittent stimuli, and its ready recovery even after considerable periods by renewed stimulations, draws attention to a peculiarity of the visual tract which plays an important part in normal vision. The rapid appearance of a uniform field, even after initial excitations which have produced a complete blocking of impulses, appears to be due to the obliterative action of impulses of normal intensity which sprea
laterally from the surrounding tracts. These impulses rapidly obscu
the effect of previous stimulation, which, but for this action, wo
produce disturbing images in the visual field.

Tests have been described which indicate the scope and utility
method of maintaining the projected image by intermittent stim
as a means of investigating visual problems and it has been sho

(1) Great individual differences exist with respect to th
cerebral tract as regards general sensitiveness, retentivit
character of the subsequent colour phenomena induced b
stimulus.

(2) A relationship appears to exist between the factors
the formation of the projected image and immediate visual

In conclusion I wish to acknowledge my indebtedness to Dr W
Rivers, and to Dr C. S. Myers, for encouragement to continue wit
early experiments. Dr W. McDougall has also kindly given m
valuable suggestions and much stimulating advice concerning lat
work. On the experimental side my father, Mr G. R. Miles, has spent unlimited patience in the development and construction of the apparatus used; whilst Mr Cyril Burt has given great assistance with regard to the arrangement and presentation of this account of my work.

(*Manuscript received* 11 *August* 1915.)

PUBLICATIONS RECENTLY RECEIVED.

Psychological Studies from the Psychological Laboratory, Bedford College for Women, University of London. London: University of London Press. 1915. Pp. 161. 2*s.* 6*d.*

This volume contains four psychological studies by various writers with an introductory note by Miss Beatrice Edgell. The problems studied are Learning and Re-learning in Mice and Rats, Controlled Association, the Nature of Recognition and Thought Processes. The investigations were carried out as part of the regular work of a class held in connection with an inter-collegiate course of lectures on 'Memory, Association, and Thought Processes.' A most valuable feature is the fullness with which the details as to the material used and as to the introspections obtained are recorded.

Individual Differences in Ability and Improvement and their Correlations. By J. Crosby Chapman (*Columbia University Contributions to Education,* No. 63). Pp. 44. New York City: Teachers' College, Columbia University. $1.00.

The general question considered in this research is the existence of general [illegible] ovability: "does it follow that the individual who gains rapidly in one mental [illegible] will also gain with corresponding rapidity in other mental traits?" The data [illegible] ere obtained during investigations made for the New York State Ventilation [illegible] sion; the primary purpose of the tests was to discover the mental effects [illegible] ences in temperature and humidity. Twenty-two men students were [illegible] on ten occasions, each with tests of Addition, Multiplication, Colour- [illegible] ancellation and Finding Opposites.

[illegible] le tests (Colour-naming, Opposites) the curves show that an approach is ma[illegible] psychological limit in fifteen minutes. In the more complex tests a much greater time is required. Even after a practise of 100 or 200 minutes improvement in addition and multiplication is still almost as rapid as at the commencement. Very high correlations between initial and final scores show that each individual tends to maintain his rank throughout with any given test. High initial efficiency seems to be accompanied by a correspondingly high power of improvement in the more complex tests, but not in the more automatic. With a much practised function, such as addition, high efficiency tends to be accompanied by a corresponding accuracy. No such relationship is discernible in such tests as cancellation. "The main contribution of the investigation is to reveal the complete inadequacy of measurements of improvement over short periods as a test of individual differences."

Educational Guidance: An Experimental Study in the Analysis and Prediction of Ability of High School Pupils. By Truman Lee Kelley (*Columbia University Contributions to Education,* No. 71). Pp. 116. New York City: Teachers' College, Columbia University. 1914. $2.00.

The problem in which the investigation recorded here originated was a practical one: how far can the mental ability of a prospective high school pupil be classified or gauged? The data used were (1) the pupils' school records, (2) the estimates of previous teachers, (3) the results of special tests. Four groups, varying in age from

13 to 16 and in numbers from 25 to 81, were examined. The tests are described, and the numerical data recorded, in full. It was found that "there can be little question as to which of the three sources is preferable...: the elementary school records give the most accurate estimate of average class standing, as well as of standing in specific courses."

Variation in the Achievements of Pupils. By CHARLES HERBERT ELLIOTT (*Columbia University Contributions to Education*, No. 72). Pp. 114. New York City: Teachers' College, Columbia University. 1914. $1.25.

The achievements of about seventeen hundred American school children in the fifth and seventh grades have been measured in arithmetic, composition, spelling, handwriting and range of vocabulary. The results are gathered into tables, which, it is suggested, may be used as tentative standards for evaluating the work of a school system in these subjects. A supplementary investigation has been made into the influence of the size of class in which children are taught. "So far as the records of these tests are valid, there is apparently no relationship between the size of class in which the children have been taught and the attainment or variability." Various reasons are discussed in explanation of this result.

Feeblemindedness: Its Causes and Consequences. By H. H. GODDARD. Pp. 599. London: Macmillan and Co. 17*s.* net.

This book is a report of work done at the Vineland Research Laboratory during the last five years, and deals with the social and biological problems involved i feeblemindedness. Pedigree charts of 327 families of 'children' at the Vinela Training School have been compiled by field-workers; each chart is supplemen by a full history of the feebleminded individual himself, and a brief account o mental attainments. From the data obtained, Dr Goddard concludes that f mindedness is transmitted in accordance with the Mendelian formula; that n mindedness behaves like a unit-character, is dominant, and is also tran according to the Mendelian law. The negative results of the investigations, those relating to commonly assigned causes, such as alcoholism, syphilis and accidents, are also of special interest. The last two chapters of th devoted to considering the treatment of fundamental problems along eu

A Manual of Ethics. By JOHN S. MACKENZIE. Fifth Editio London: W. B. Clive (University Tutorial Press). 1915. Pp. xxii + 500. 6*s.* 6*d.*

The fifth edition of Professor Mackenzie's well-known manual contains se important changes, involving the addition of nearly fifty pages. Many of t due to the writings of Mr G. E. Moore. The chief is the addition chapter on 'The Further Determination of the Moral Standard' and a brief in both of which Mr Moore's views are discussed, mainly with approval. Furthe 'Notes' have been appended to certain chapters, dealing briefly with 'Recent Views about Pleasure' (mainly the views of Sidgwick, McTaggart, Rashdall and Moore) and 'Theories of Development' (mainly the theories of Spencer, Green and Nietzsche). References for further reading are brought up to date. Recent psychological work bearing upon the problems discussed is but occasionally mentioned.

The Principles of Judicial Proof. By JOHN H. WIGMORE. Pp. xvi + 1179. Boston: Little, Brown and Co. 1915. 30*s.* net.

In this volume Dr Wigmore discusses the Principles of Judicial Proof 'as given by logic, psychology and general experience, and as illustrated in judicial trials.' He considers first the generic human traits affecting the trustworthiness of evidence; and discusses in detail the influence of age, sex, race, mental disease, moral character, feeling, emotion, bias, and experience. He then minutely analyses the actual testimonial process. His method is to distinguish and classify the various psychological factors concerned; and to devote a separate section to each factor, citing at length relevant excerpts from leading psychologists and jurists, and appending law cases in illustration of the principle discussed. The book includes a general account of the experimental investigation of testimony; and, as a whole, is of considerable value to those interested in the psychology of evidence.

The Vicious Circles of Neurasthenia and their Treatment. By JAMIESON B. HURRY. Pp. xv + 90. London: J. and A. Churchill. 1915. 3*s.* 6*d.* net.

In a series of ten brief chapters the author discusses the 'vicious circles' associated with psychoses, with the sense-organs, and with the vascular, respiratory, digestive and genito-urinary systems; and concludes with a longer chapter on 'The Breaking of the Circle.' The circles and the points at which they may profitably be broken are illustrated diagrammatically in a series of five plates.

Psychology and Parenthood. By H. ADDINGTON BRUCE. New York: Dodd, Mead and Co. 1915. $1.25 net.

The writer has endeavoured "to review and unify, in non-technical language, the findings of modern psychology which bear especially on the laws of mental and moral growth." His illustrations are often of interest.

Mind, Nos. 93, 94 and 95. London: Macmillan and Co. 1915. 4*s.* each.

he Philosophical Review, Vol. xxiv, Nos. 2, 3, 4 and 5. New York: Longmans, Green and Co. 1915. 3*s.* net.

The Journal of Animal Behaviour, Vol. iv, No. 6, Vol. v, Nos. 1, 2 and 3. Cambridge, Boston, Mass.: Henry Holt and Co. 1915. Subscription $3.00 per volume.

The Psychological Review, Vol. xxii, Nos. 1, 2 and 3. Princeton, N. J.: Psychological Review Co. 1915. Subscription (with *The Psychological Bulletin*) $5.00.

The Psychological Bulletin, Vol. xii, Nos. 1, 2 and 3. Princeton, N. J.: Psychological Review Co. 1915. Subscription $2.75.

Bulletin de l'Institut Général Psychologique, 14ᵉ *Année*, Nos. 4—6, 15ᵉ *Année*, Nos. 1—3. Paris: Au siége de la Société. 1914, 1915. *Abonnement annuel*, 20 f.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

May 8, 1915.	Some Effects of Training on Children's Powers of Observation, by Miss I. B. SAXBY. Observations on a Three-legged Dog, by A. WOHLGEMUTH. The Oscillatory Character of Attention, by J. C. FLÜGEL.
July 3 and 5, 1915.	Joint Meetings with the Aristotelian Society and the Mind Association.
July 3, 1915.	Mr Bertrand Russell's Theory of Judgement, by G. F. STO
July 5, 1915.	The Import of Propositions (Symposium), by Miss CONSTANC JONES, BERNARD BOSANQUET and F. C. S. SCHILLER.

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STEREOSCOPY

AS A PURELY VISUAL, BISYSTEMIC, INTEGRATIVE PROCESS.

BY HENRY J. WATT.

I. *The integration of stereoscopy cannot include any quality other than the visual.*
II. *Stereoscopy cannot be founded upon the integration of differences in any single attribute except systemic order, and not directly even upon that.*
III. *Exclusion of temporal modes and therewith of motion.*
IV. *The integration of stereoscopy must rest proximately upon bisystemic differences of distances or forms.*
V. *Form in relation to the origin, maintenance, and alterations of binocular correspondence.*
VI. *The role of form in the process of uniocular stereoscopy.*
VII. *Theory of stereoscopic vision:*
  (a) *The rules of bisystemic disparity.*
  (b) *The negative case of 'vertical' disparity.*
  (c) *The systemic sign.*
  (d) *As a process, stereoscopy is a bisystemic, and therefore purely visual, integration of bidimensional forms; as a product, it is a purely visual tridimensional form, not space.*
  (e) *The interplay of primitive stereoscopy and higher determining processes.*

STEREOSCOPIC vision is admitted to be a complex kind of sensory experience. A complete theory of it presupposes knowledge of its relation to its physical stimuli, of its dependence upon neural processes, and of its constitution as a complex experience. The first of these three fields of knowledge displays the adequacy and efficiency of stereoscopic vision, but does not offer any paths towards true theory.

The problems of theory are, of course, set solely by the peculiar nature of the experience; but their solution may be sought either within experience itself or in the sphere of neural processes. The quest of the great majority of present day theories does most certainly run in the latter direction. The view prevails that a purely psychological theory of stereoscopic vision is more or less impossible or chimerical.

The popularity of physiological references rests upon a double basis. On the one hand nerves and neural processes are accredited and substantial parts of the material world. On the other hand we do not seem to find in stereoscopic vision any traces of those processes which a theory of the derivation of stereoscopic vision from simpler visual experiences would lead us to expect. Stereoscopic vision seems to spring fully grown into life. Besides an explanation of stereoscopic vision by reference to simpler visual experiences would somehow suggest the manufacture of something out of nothing; two dimensions somehow turn into three. Of the latter two reasons recent investigations have shown that the former is actually groundless. Traces of processes can be detected. And the latter reason really puts every would-be complete physiological theory of stereoscopic vision out of court. For we cannot expect the complications of the material or biological world to explain for us the occurrence of something specifically new; unless, indeed, we adopt the naïve point of view that the collocations of neural matter which underlie our sensory experiences are exactly like, and therefore suffice to explain, the processes of experience with which they are correlated. Surely there are few to-day whom such an 'explanation' could satisfy, even if they forget that neural processes are never supposed to possess states of feeling or thought over and above the feeling and thought they evoke in our 'minds.'

In this paper I shall endeavour to increase the hope and promise of a psychological theory of stereoscopic vision by advocating certain plans of systematic exposition and by removing certain more or less permanent barriers to insight and progress. Stereoscopic vision is so complex that it can never be made to appear simple; but our knowledge of the facts and our insight into the principles of its construction enable us to grasp it at least as a scientifically continuous process[1].

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[1] [The work of this paper originated in my desire to obtain a full and satisfactory theory of stereoscopic vision in pursuance of my efforts indicated in my paper: "Some Problems of Sensory Integration," This *Journal*, 1910, III. 323 ff. esp. 331 f. For this purpose I found it necessary to construct for myself a pure psychology, adequate in principle, if not in detail, to all kinds of sensory experience simpler than stereoscopic

I. *The integration of stereoscopy cannot include any quality other than the visual.*

(*a*) The theory has been advanced that sensations of muscular quality from the extrinsic and intrinsic muscles of the eyes fuse with visual sensations to give stereoscopy. This view is based upon certain correlations between objective conditions of the eyes and variations in stereoscopic vision or at least judgments regarding distances in the line of sight. In the case of judgments correlated with changes of accommodation it cannot be denied that they are devoid of any basis in stereoscopic vision; the object is judged, but not seen stereoscopically, to be nearer or farther; the question then arises as to the basis in experience upon which these judgments are founded. It has been urged, for example, that this basis consists of the visual effects produced by voluntary changes in the direction of the attention[1]. But this question may be left for separate treatment at the proper place. In the case of judgments correlated with changes of convergence the latter produce in ordinary daylight vision such changes in visual presentations as undoubtedly afford a better basis for stereoscopic integration than could sensations of the accompanying muscular changes. And it is not disputed that the exclusion of all changes except the muscular ones of convergence reduces the functional efficiency of vision far below its usual level. The high stereoscopic efficiency of ordinary vision cannot therefore be due to the cooperation of muscular sensations. Nor is the muscular efficiency of the eye itself improved to the required degree by the circumstances of ordinary vision. The investigations of Judd and his pupils have shown how irregular and rough the work of the eye muscles is, compared with the accuracy of visual discrimination in any dimension. In comparing the two cases we have to note that judgments correlated with changes of convergence alone are much more accurate than those correlated with changes of accommodation. But even in the former there is evidence to show that the

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vision, and then of course to the latter also. The problems of stereoscopic vision have been constantly in my mind, and their solution has developed over what often and long seemed to me to be insuperable difficulties and mysteries. But yet, once the main principles of the task became clear, I never doubted, as I do not now, that a pure psychology would be able to answer fully all the questions to which answers may legitimately be expected from a science of sensory experience. The papers referred to occur in This *Journal*, iv. 127 ff.; v. 292 ff.; vi. 26 ff., 175 ff. and 239 ff.; vii. 1 ff.]

[1] Cf. E. R. Jaensch, "Über die Wahrnehmung des Raumes," *Ztsch. f. Psychol.* 1911, Erg. Bd. vi. 135 ff., 140 f.

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judgments are not based on that particular experience that is commonly called stereoscopy. This stereoscopy therefore cannot include muscular sensations within its integration.

In spite of this and similar damaging evidence, however, the theory of the cooperation of muscular and visual sensations in stereoscopic vision in general maintains its acceptance with many[1]. This is undoubtedly due to the fact that the usual 'point theories' of stereoscopic vision really fail to offer any satisfactory explanation of it. They fail especially to explain the effects of experience (in the broad sense) upon vision. So recourse is had to muscular factors, since all 'experience' obviously involves these. We must therefore examine the psychological possibility of this theory.

(*b*) The integration of muscular with visual sensations is psychologically unthinkable. Consider first the qualitative attribute. The fusion of qualities is familiar in the senses of vision, taste, and smell. Of course we may consider the differences of quality which occur within each of these three[2], as either differences of quality within a single sense or as differences of quality within groups of qualitatively closely allied senses[3]. We should then admit the possibility of fusion between the qualities of one and the same sense or between senses of closely allied or similar qualities. No one will suggest that the visual and muscular qualities fall under either of these two heads. Thus far at least we are not encouraged to assume the possibility of their fusion.

But fusion between qualities of different senses is also alleged. Taste and smell are held to blend so intimately that only experimental isolation can give a proper classification. This is doubtless true as it stands. Without experiment we cannot readily tell savour from flavour. But it is not asserted that tastes disappear in smells or vice versa, so that from the fusion a new kind or feature of smell or taste arises. The problem of the introspective similarity or difference between the qualities we now know to belong to taste on the one hand and to smell on the other hand would hardly have been grievously retarded, even had we been unable to isolate the senses of taste and smell. Fusion seems thus to be restricted to allied or similar qualities.

Thus we find that we can assume no analogy for the fusion of visual and muscular qualities. The only other adequately known sense of equal rank with vision—the sense of sound—shows no fusion with the qualities of any other sense. Fusions between vision or sound on the one hand and touch or muscular sense on the other hand have been postulated often enough in order to explain the bisystemic products of these senses—stereoscopy and the localisation of sounds. But they have never yet been proved. And they are seriously discredited by the vicious mental chemistry they postulate. The minor fusing sense of touch or muscularity is supposed to disappear in the product and to give rise to a new feature of experience which attaches itself solely to the fusing experiences of vision or hearing. Experimental isolation cannot be supposed to restore the minor partner of the fusion in these cases as clearly as the less obvious sensations are restored to notice by the isolation of taste and smell. And, besides, in the former cases the sensations thus restored cannot carry out in isolation the distinctions they are held responsible for in the fusion, as can the isolated qualities of taste and smell.

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[1] Cf. e.g. E. B. Titchener, *A Textbook of Psychology*, 1910, 314 and 334.

[2] The fusion of cold and warmth may be placed alongside them.

[3] Cf. This *Journal*, 1914, VII. 3.

(*c*) Theories which accept a fusion of visual and muscular sense do not usually enquire at all as to how the fusion comes about. It is enough for them that there seems to be a need for postulating fusion and that on both sides—muscular as well as visual—some sort of variation occurs. We cannot afford to be so careless now. If a fusion of qualities as such is highly improbable, it is quite hopeless and useless when we consider the extent of variation in qualities that occurs. Strictly speaking, we must admit that muscular sensations do not vary in quality at all[1]; only one quality of this sense occurs. The muscular sense, through its attribute of quality, is therefore quite incapable of producing the manifold variations of stereoscopic localisations which have to be explained. This statement is almost equally true of the qualities of vision. For all possible stereoscopic variations of localisation can be produced with but a minimal or single variation of quality on the visual side, if we admit for the moment that differences of grey-brightness are to be taken as qualitative. An integration of quality is therefore excluded on both sides.

Passing now to a consideration of the attribute of order, we may readily admit that the variation on the visual side is sufficient for all purposes in question. But it is not so on the muscular side; in fact muscular sensations can hardly be supposed to vary in order at all except from muscle to muscle; within one muscle their only variation

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[1] This *Journal*, 1911, IV. 146, 148; 1914, VII. 3.

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is that of intensity[1]. How then should the muscular sensations from the eyes form a parallel to the orders of the fields of vision and lead to their differentiation into a new set of orders of the third dimension? Neither could a series of intensive differences in any sense, even a highly discriminating sense like sound, nor the minimal differences of extensity and therefore, by implication, of order also, which a physiological theory of intensity[2] might postulate, or induce us to read into the variations of muscular sense, form an adequate correlative for the stereoscopic modifications of the orders of vision. Apart from all this lack of variation it is highly improbable that *different* attributes of two senses join or fuse with one another to form an addition to the attributive features of one of these senses[3]. We have no clear analogues of such fusions in well-known sensory experiences and their very occurrence would present us with something irrational and inexplicable. We can expect elementary sensations to merge into one another only when their qualities fuse. If their qualities do not fuse, the sensations themselves must stand apart, more or less easily distinguishable for introspection according to the degree of prominence with which they enter into cognitive processes.

But if their fusion is excluded, their correlation and connexion is not also rendered impossible. We know, for example, that sight and sound do not fuse in quality. The very suggestion appears absurd, so clear is the introspective distinction of these qualities. But the orders of vision do nevertheless become connected and correlated with the cognitively parallel localising orders of hearing. We hear a sound and see in imagination the place it comes from. We 'associate' the sound of the voice with the sight of the moving lips so closely that we can be misled by the habits we have formed, as ventriloquists show us But here one fact is quite obvious. These cases do not present to us any addition to the sensory experience of either correlated sense, vision or sound. The bell whose sound is located by reflection at some false point is not therefore seen at that point or with some new process of localisation. Nor does the stick that is bent in water look straight, when we put our hand into the water and feel that it is straight. To be sure some addition to experience does result from the correlation of sensory systems; but this addition does not belong to, or fall within, the scope of any single sense. If muscular and visual senses do become connected in this way, their connexion cannot account for the origin of stereoscopic indexes, which do most certainly fall entirely within the scope of visual experience.

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[1] Cf. *loc. cit.*

[2] Cf. C. S. Myers, This *Journal*, 1913, VI. 137 ff.

[3] Cf. This *Journal*, 1911, IV. 150.

What holds good for muscular sensations, of course holds good all the more for muscular imagery. For memory is always weaker than sense, and such a wealth of muscular imagery as would be required does not exist.

(*d*) One of the most decisive facts of stereoscopic vision is the complete reversal of the whole system of depth relations that ensues with geometrical figures when the left and right eye pictures are interchanged in the stereoscope. If this reversal is to be explained with the help of muscular associates, we must suppose that an association or determination of some kind is formed between the visual complexes and the muscular sensations presented to each eye respectively. But if such an association is to be formed, some psychical feature must exist which individualises the visual experiences of one eye from those of the other. For without that, interchange of left and right eye stimulations would produce in the total complex of psychical presentations no change whatsoever. Experiences cannot be just as a matter of fact associated, if association is to make a noticeable difference within the total experience. Subconscious or physiological connexions or associations would, of course, suffice for the mere linkage of experiences into groups; that is to say, they might well determine the time of appearance of experiences. But they could not determine the 'arrangement' of these experiences amongst themselves or differentiate from one another experiences which are otherwise identical. In order to do that they would have to supply each of the experiences with a conscious mark or sign. This sign would be given either before or after the appearance of the experiences in consciousness.

If the sign is to be given afterwards, it is presupposed that no psychical connexion or association has been established before the sign appears. It is then not evident how the experiences which are to receive the sign are to be distinguished from the other experiences which may be present, so that the conscious connexion may be established and the sign received[1]. If the sign is to be determined before the experience in question appears, it is presupposed that the connexion or association has also been established before the experience appears. This may, of course, be the case in vision. But if it is, it would be difficult to resist the conclusion that the visual experiences received their sign from their own specific nature as visual experiences, and apart from any subconscious or physiological connexions with senses other than visual. In any case the occurrence and results of such preconscious connexions are a subject only for special physiological research and do not fall within the scope of pure psychology. And the further work of integration in consciousness would be from that point onwards purely visual. In both alternative cases then the cooperation of conscious muscular sensations with visual sensations to produce the effects for consciousness of the interchange of uniocular impressions is not required. For in both cases we require to postulate in the visual experiences some distinguishing mark or sign by which their varying association with other variables shall be possible. I have called this index the systemic sign[1]. If each field of vision is marked by a systemic sign, it is unnecessary to appeal to simultaneous muscular sensations for an explanation of the effects of reversals upon consciousness; for these reversals could be equally well explained by the interchange of the systematic signs or of the contents of the two fields of vision. And if we do not need the help of muscular sensations for the explanation of reversals, the presumption is that we do not need them for the explanation of stereoscopic vision at all.

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[1] Cf. *loc. cit.*

The refreshing effect of a change of convergence upon stereoscopic vision may well be due to some visual process which results from a change of convergence. This is indeed very likely upon purely negative grounds; for why should the change of convergence be necessary at all, if muscular sensations do really enter into the integration of depth? The afferent impulses which are responsible for the maintenance of posture cannot be supposed to lapse because of any adaptative process. Otherwise the posture itself would give way. We need not therefore suppose that the sensations from the eye muscles lapse because of accommodation, although they may well cease to be the object of attention. And yet such an hypothesis is not compatible with the assumption that they actually enter into conscious integration with visual experiences, to which we are supposed to be attending specially in binocular vision. We may then suppose that the posture of the eye is maintained by its own postural mechanisms without the aid of consciousness, and that changes in that posture are due to interference from visual sources. The posture of the eye is changed according to the requirements of vision.

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[1] This *Journal*, 1912, v. p. 303.

We have thus every reason to believe that the integration of stereoscopic vision is purely visual and in no way involves any fusion with sensations from the muscles of the eye.

(*e*) All the above arguments apply equally to any theory that assumes the integration of labyrinthine and visual sensations. Whatever conscious form they may be supposed to take, the sensations evoked by stimulation of the labyrinth are not of sufficient variability to provide a functional correlative for the enormous variability of stereoscopic vision. Qualitatively the integration of these two kinds of sensation is psychologically as unthinkable as is that of muscular and visual sensations. An integration of orders is perhaps more conceivable in the case of labyrinthine sensations, as these may be supposed to vary only in order. But their integration would necessarily be so restricted—to directions within the frontal plane that uniocular vision supplies—as to be insufficient for the needs of stereoscopic vision. And if the system of labyrinthine sensations be supposed to be permanently present (*i.e.* 'conscious') and tridimensional, it might be looked upon as a sort of original spatial *Anschauung*, or Kantian 'form,' lending itself to the material of the other senses. But this theory would raise again the problems of psychical banking, and it is our confirmed opinion that no safe business can be conducted unless the borrower can display a steady basis for credit in his own possessions or powers. We should then have to suppose that vision itself is tridimensional, whereby the ground of our appeal to any other sense would have disappeared. It cannot be too often insisted, in face of the endless unfounded assumptions that have been made by psychologists in dealing with these matters, that we can expect a sense to participate in an integrative correlation whereby it is to receive some new mark or sign, only to the extent of its own original variability, and that the resultant variability is not by the least jot or tittle greater than the original; it has only entered into a new system to which it is through its own original variability, and by each point of that, attached, and by whose content it is point for point enriched. So no correlation with any non-visual tridimensional system will make the visual system tridimensional. The visual system must be of itself tridimensional. But integrative correlation can give visual tridimensionality a new 'significance' or modification; it may convert it from what it is of itself—tridimensional form—into what it is for our common sense —tridimensional space (cf. below VII. (*d*)).

In recent years a body of evidence is gathering to show that labyrinthine sensations do not exist; the experiences associated with the term seem rather to be muscular in origin. If this is really so, the argument simply reverts to that applied to muscular sensations. In any case, therefore, no help towards the solution of the problems of primary stereoscopy may be expected from labyrinthine sensations.

II. *Stereoscopy cannot be founded upon the integration of differences in any single attribute except systemic order, and not directly even upon that.*

1. It is unnecessary to repeat the arguments[1] which exclude any appeal to the five attributes other than order. No one would think of appealing to them directly as the source of stereoscopy. Only those who feel that for *simple* systemic order or local sign a source or origin must at all costs be found, ever turn to such a refuge of desperation. Bunches of qualitative and intensive differences, which are not even probably adequate as mere correlatives for the actual differences of local sign, cannot by any device be thought to convert themselves into local signs.

2. Against a direct appeal to the attribute of order and *pari passu* against the physiological theory that depth is derived from the integration of bisystemically disparate points, the following arguments may be urged:

(*a*) The mode of depth is in many cases not perfectly stable. It can be destroyed by staring at one of the points shown, e.g. at the nearer or farther of two points in the fundamental stereoscopic slide. After a time the stereoscopic distance between the points vanishes; and double images of the point, that has not been fixated, emerge. We are then aware of the position of each of these images and of the distance between them. But by staring at one point we do not affect the primary orders of the images of the other point; for these orders are fundamental and unchangeable; if depth is integrated from them, it should not be affected by staring. It might, however, be said that depth is destroyed in this case because we have withdrawn from the integration of the depth-order of the other point the support it usually derives from an evenly distributed attention; we must distribute the attention over the field of vision, if the two points are to come into depth relations to one another. But does not this argument imply the assumption of the existence in the depth integration of something besides mere orders and their differences, of something that integrates intrasystemic differences of order, in a word, of the simple form of distance across the line of regard?

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[1] Cf. This *Journal*, 1911, IV. 138 f.

Outside the limits of stereoscopic fusion we are normally aware of double images and of their relative orders. Stereoscopic vision, therefore, practically ceases to exist where differences of order become most clear and distinct.

(*b*) The theory under discussion must admit, not only that each visual point has its own intrasystemic order, but that to any point in one system there is one and only one point in the other system which with the first will give single vision. Consequently there must be within the fovea of each eye an absolute centre of reference for the definition of bisystemic correspondence and disparity; and this centre of reference must be functionally unalterable.

The careful investigation of eye-movements during fixation made by C. N. McAllister[1] shows that no such absolute centre of fixation and of functional reference can exist: "The image of a point fixated does not fall upon any particular point of the retina, but may fall upon any point of a considerable area of the retina, around about, and including the fovea centralis. The same elements of the retina are not stimulated during any successive periods of fixation of a point, except by mere chance." "The movements made in changing fixation from one part to another of the field of vision are not the same for any two successive movements." "The movements of the two eyes during any period of fixation, or in passing from one point to another in the field of vision, are not coordinated."

The hypothetical centre of reference has been found to be altered in certain cases of squint[2].

If it be said in reply that, of course, there is no absolute centre of reference, but that the form of the objects seen is also involved in binocular identification and integration, emphasis must be laid on the fact that the identity of form implies something more than the identity of mere orders (apart from the accompanying identity or, as it may be, difference in respect of the other variable attributes). Besides, how shall we prove identity of form, apart from the assurances of common sense, if we have to admit a variable bisystemic correlation of orders?

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[1] *Psychol. Rev., Monogr. Suppl.* 1905, VII. (1), 52 f. Cf. C. H. Judd, *ibid.* 1907, VIII. (3), 380. R. Dodge, "An Experimental Study of Visual Fixation," *ibid.* VIII. (4), 21 ff.

[2] Cf. Asher and Spiro, *Ergeb. d. Physiol.* Abt. II. 1st Year, 1902, "Die neueren Untersuchungen uber das Sehen der Schielenden," by F. B. Hofmann.

We cannot here appeal to a mere recognition of the facts, however emphatic the recognition may be. For the point of the argument is that the theory adopted implies one particular set of facts, which were at least once held to be true. That set of facts cannot be expanded, not even with the help of various subsidiary hypotheses, such as that of the role of the identity of form just mentioned, without the destruction of the original theoretical framework. The reference to form cannot be introduced as a subsidiary hypothesis. Form must be recognised as the prime basis of stereoscopy.

III. *Exclusion of temporal modes and therewith of motion.*

We turn next to experiences other than simple sensations and their attributes. Of these the mode of depth is itself one. Depth, therefore, can be derived only from modes of experience that are simpler than itself. Of these there are, apart from processes of identification, only three groups: distance or systemic form, time-interval or temporal form, and motion. No one would think of referring to time-interval for an explanation of binocular static stereoscopic vision. Motion, however, presupposes and involves both time-interval and distance, so that the exclusion of the former prevents any appeal being made to motion and leaves us only the integration of distance. If motion seems to be effective, it can only be so in virtue of the distance it involves, and that must be capable of acting by itself. Thus we arrive at the fourth thesis.

IV. *The integration of stereoscopy must rest proximately upon bisystemic differences of distances or forms.*

(*a*) The mode of depth resembles that of distance more than it resembles the attribute of order. Intrasystemic orders are given in a certain form and are essentially unchangeable. A single sensation has its order whether others are given along with it or not. But in the case of the derivative depth-orders this is hardly so. The occurrence of several together makes each enormously clearer and more effective. It is hardly an exaggeration to say that depth is specially given only when several points are presented at different distances in the line of sight[1]. To describe this the expression 'relative depth localisation' has been used. The localisation of every point is said to be given by direct reference to the point of fixation, no matter where this lies. This point of fixation has, therefore, in a certain sense, the depth value 0, because all other points have either the same value as it, or they deviate from it in opposite directions, which we know as nearer to ourselves or farther away. It must be evident, however, that no theory that ignores the part played by distance in the origin of stereoscopic vision has any right to consider depth localisation always relatively to the point of fixation, either in a physiological or in a psychological sense; it may do so only in a scientific sense, for purposes of systematic statement. We may therefore consider depth as primarily a kind, or modification, of distance[1]; no doubt a new system of orders emerges therefrom.

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[1] Cf. L. v. Karpinska, *Ztsch. f. Psychol.* 1910, LVII. 44, and the paper by Petroniewicz as there quoted by her.

(*b*) Distance (or its complexes of *form*) seems to be a sufficient medium for the identification and integration of heterosystemic presentations. For it is a familiar fact that intrasystemic distances and forms can be readily compared with one another apart from the identity of the orders which integrate to form them and without a comparison of orders or inference from the conceptual treatment of orders[2]. If we suppose, as we may, that the two visual systems given by the eyes were originally of similar construction[3], the stimulation of the two retinae by the same object would produce in each system a complex of distances, *i.e.* a form, which would often be very similar in the two systems, and would always change in both simultaneously in highly similar ways. Attention to the one would unfailingly bring about attention to the other form, and so a basis for a highly unitary act of vision would be given. Some sort of bisystemic correlation of points or orders would, of course, be of great advantage in this act of vision, as we shall see later; but it would not be indispensable. It is at least not a presupposition, but far more probably a result, of unitary binocular vision. Besides an absolute bisystemic correlation of orders would be useless for the purposes of stereoscopic vision. It could not yield the stereoscopic localisation of a single point in space, but only mere bisystemic identity.

If distance or form were the medium of identification, incoordination of eye-postures or of eye-movements during fixation or inspection of a figure would not necessarily detract from the ease and efficiency of binocular vision or from its unitariness, unless the incoordination exceeded a certain amount.

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[1] This distance, being merely the intrasystemic integrate of order, has as yet no spatial connotation

[2] Cf. This *Journal*, 1911, IV. 174, 179.

[3] Doubtless they *acquired* this similarity of construction under considerable psychical leading.

Form seems, finally, to be the medium whereby the visual continuity of successive fields of vision is maintained. Here the eye, in practically all cases, shifts its point of fixation suddenly, while no peripheral addition to perception is made during the movement. Part of the contents of successive fields of vision is identical, but these contents stand at different positions in the system in successive fields. The identification and the absence of apparent movement is only possible because of the identity of form and the more or less entire absence of visual motion during the fixations of the eye, aided by the high speed of eye-movement and the accompanying lapse of perceptual additions.

(*c*) The mode of depth would thus be integrated out of bisystemic differences of form, or, at the simplest, of distances. Staring at one of two points might, then, readily lead to such an absorption of attention in mere order[1] that the mode of depth would be destroyed by the weakening or disruption of the integrating distances. The differences of order of the points shown would become apparent, so much so that we even become aware of the distance between double images, as we never do in ordinary stereoscopic vision proper. A change of fixation and convergence would, however, at once distribute the attention properly over the points and their distances, and the mode of depth would be at once realised.

(*d*) One explanation would suffice both for binocular and for uniocular stereoscopy. This would not be the case if the basis of integration were disparity of retinal points or bisystemic differences of order. For there is in uniocular vision no analogue to the correspondence assumed for binocular vision. There is not even a true point of fixation, when the eye is in motion; for the eye cannot be relied upon to keep its motion in such close harmony with that of a moving object or impression as to ensure the maintenance of an absolute point of reference for uniocular stereoscopy, from which the disparity of the stimulations of successive moments might be measured. But if an integration of forms be the essence of stereoscopic vision, the disparately changing forms of successive moments might well suffer some degree of irregular collective displacement upon the retina or within the uniocular system without stereoscopic vision being thereby in any way disturbed. The identity or disparate irregularity of the forms of successive moments vould merge into integration without contamination by any simul- aneous total shift of position within the uniocular system. Any total isplacement would easily be discounted from the other regular or isparate changes going on within the complex of forms given by timulation.

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[1] Or, as in most ordinary cases, mere identity of minute restricted units of form.

In binocular vision the distinction of motion in the frontal plane rom stereoscopic position is inevitable; for the latter involves no progressive change of orders at all. Any motion in the line of sight is formed stroboscopically from the stereoscopic positions of successive instants. In uniocular stereoscopic vision the matter is a little more complicated; for both ordinary motion in the plane perpendicular to the line of sight and stereoscopic vision are constituted by the change of orders of uniocular points of successive instants. But the distinction cannot be difficult. For in ordinary motion the whole form representing an object is displaced in a perfectly uniform way, in so far at least as the object presents a plane superficies to the eye. In so far as the object is solid, the nearer points of it will be displaced in successive moments relatively to the far points according to a definite rule. Only those relative displacements within a total form that follow this rule will evoke stereoscopic vision; all other displacements that are regular for the whole figure will be regular, *i.e.* will give no complication in virtue of the successive series they form from moment to moment, and will, therefore, be taken as simple total motion of the object.

Moreover, for all stationary objects there will be a constant system of relations between the rate of translation of the eye or body of the animal—fish or bird—and the apparent relative depth distances of objects. And the moving animal must be aware in some way of its rate of translation; for if it were not, its stereoscopic vision would be ambiguous; the stereoscopic index, as it were, would be lacking; for one and the same set of objects quicker flight would give a higher rate of change of disparity, and that, apart from some controlling factor, would appear as a greater plasticity. Similarly a slower rate of flight would appear as a smaller degree of plasticity. Of course the animal's awareness of its rate of flight need not be very fine, any more than is our awareness of the degree of convergence of the eyes. But it seems evident that it must be present in some form. If so, then whatever conforms to the system of relations between rate of flight and disparate changes of form will have a certain 'absolute' plasticity. Motions in the line of sight will, of course, be formed uniocularly after, and by means of, the stereoscopic determinations of successive instants.

These statements do not presuppose the presence of a logicall discriminating intellect, however much the appearance of such a intellect may contribute towards the psychical efficiency of sensor experience. Our argument implies only that motion would be possibl with the presentation of progressive variations in order alone, but tha stereoscopy implies the presentation of differences of distances, an that the distinction of motion and stereoscopy is merely another aspec of their different psychical origin and essence; the same applies to any 'distinctions' which awareness of the rate of translation of the body may make possible, as indeed to every specific integration, whether of sense or of intellect or of any other sphere, except in so far as it may be aided or hindered by other (integrative) psychical processes. Psychology must avoid the anthropomorphism of the intellect, which in the past has so constantly hindered its progress and obscured its real problems.

V. *Form in relation to the origin, maintenance, and alterations of binocular correspondence.*

(*a*) We cannot assume an original correspondence of the two eyes. To do so would be, of course, to beg the question entirely. Two anatomically distinct organs cannot be supposed to come into being in subtle harmony with one another, unless some uniting influence has constantly operated upon them to produce this harmony. Various indications guide us towards a discovery of this moulding influence.

As we find them at present, the eyes do not entirely correspond to one another. They do indeed fit into, and work with one another, perfectly. But they do not completely coincide or overlap. They are still largely independent systems forming part of a larger system, just as the various touch organs in the skin join to form a single large system. So we may suppose the primitive optical organs also to have been optical points, all of equal value, but of different order, forming a single system. Now we find all these primitive optical organs or their derivates bunched together into two separate but coordinated systems. The stimulation of points in purely uniocular parts of each system gives the awareness of an optical distance between these points, just as does the stimulation of two points in the large sensory system of the skin. Relatively to the two uniocular portions of the field of vision, it is a mere accident that the rest of it is binocular; it is not absurd to suppose there might be no binocular part at all, so that the two uniocular fields should just 'touch' to form one plane system, or should form a system in which they were separated by a constant distance. A coordination of minor systems to form one system, therefore, does not imply correspondence.

Thus we see that, as they are now, the two eyes are only partially in correspondence with one another. This actual correspondence will, therefore, not have been original, but derived, and must still rest upon some purely psychical basis, by which its existence is also maintained. Its actual maintenance is made obvious by the fact that the correspondence that has been established is at least to some degree alterable, as the facts regarding the vision of squinters have shown. Of course we may readily admit that the basis of correspondence may at any time be largely physiological and yet maintain that the possibility of, and the first approach towards, correspondence was given in psychical terms. Physiological correspondence may be of great service in ensuring for each individual those conditions of vision that are most advantageous for the rapid re-acquisition of what has been gained by his ancestors, and in ratifying for him what he has gained for himself. But they will only ensure and ratify these indirect conditions of binocular stereoscopic vision; they will not themselves be the direct basis of the complex sensory experience specific to stereoscopic vision. On the other hand any such complex experience forms such a unique addition to mental life and brings such new and valuable insight with it that its occurrence must reflect with great vigour upon the biological stability of all circumstances which favour its occurrence. Only in mental life do we come face to face with the inner essence of forms of complication. And in mental life we can see how the coherence of the higher state is not only made possible by the simpler processes of which it is a complication, but reacts upon them to give them a greater permanence and stability than they have of themselves. Thus do higher processes lead and strengthen the lower.

(*b*) The same course of thought applies to the problem of the relative position of the two eyes. We cannot assume as original any such position as is bound up with some particular psychical advantage. We can only suppose that, the conditions for the formation of two eyes being given, the accidental approximation to certain positions brought about such results in experience as sufficed to react upon the conditions circumstantial to the occurrence of these positions and to render them more permanent. From the point of vantage thus gained any other variation in the line of greater psychical advantage would be highly favoured.

There are two such lines of increasing advantage, one for animals habitually in a state of motion, the other for those that are either habitually at rest or that have constantly to determine their course of motion while they are still really or virtually at rest. For the former the greater advantage would induce an approach towards a common axis of vision; for to the natural occurrence of stereoscopic vision under conditions of progression there would be added the advantage of a maximum field of vision, a field so large that it would include the whole visual panorama. For the latter, *i.e.* for carnivorous, or generally, for leaping animals, the greater advantage would induce an approach towards parallel axes of vision, which would secure an unbroken field of vision without movement of the head out of the direction of leaping. But this forward vision would be accompanied by projection of certain objects upon both retinae. In consequence of the identity or similarity of forms or of change of forms thus produced, the perceptual values of the complexes common to the two retinae would be the same, and the values of the primary visual orders occupied by the same objects would be identified. But this identification would not, of course, affect the primary values of these orders within the primary systems. These would remain fundamentally the same, although modified by a new aspect due to the bisystemic identification.

(*c*) But for each state of adaptation of the bisystemic angle of visual regard a standardisation of the two systems would be then required. The disadvantages accompanying a want of some kind of correspondence between the eyes, for all animals whose eyes are movable, would be very great. For without a fair degree of correspondence the muscular responses of the eyes could not be brought under proper coordinated control. Even the single eye would have no natural method of adopting a posture; fixation would follow no definite plan or rule, but could only vary eratically from moment to moment. It would then be impossible for the eye to distinguish correctly between displacements of forms in the uniocular system due to the movement of the objects of vision and displacements due to the movement of the eye itself[1] over against stationary objects. A point of fixation forms a centre of coordination for the two variable series of correlates—

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[1] The chief point in considering the relation of eye movement to such 'perceptions' is the fact that the eye moves almost entirely under the control of (visual) experience, not of itself, and that, when it does move 'of itself,' *e.g.* when the head is subjected to vibration or reflexly from the semicircular canals, experience displays motion in vision. But the text does not imply any discussion of this relation.

movements of objects and movements of the eye itself—and by its help the complex of displacements of the visual forms within the visual system can be readily distinguished into its objective and subjective, or eye movement, components.

The optical instrument of fixation is the area of clearest vision in the foveae. This area is not only of value as yielding the clearest vision, but it must also be of enormous importance as a physiological means of ensuring the visual correspondence and the muscular correspondence of the two eyes. It must be obvious that an area of clearest vision can only arise in so far as the degree of greater clearness already attained makes it possible for the eye to be more efficiently controlled muscularly. This greater control would thereupon react upon the area of clearer vision, allowing it to be used oftener for minutest visual work and so to yield the full advantage of every increase of acuity of vision that may occur in it. The acuity of vision in the fovea and the muscular control of the eye would therefore evolve in equal measure.

We seem thus to be led to the conclusion that the differential acuity of vision of the foveae and the rest of the retinae is the basis of the natural correspondence of the two eyes; it must be the vehicle for the transmission of that great aptitude and determination towards the bisystemic identification of forms and towards the integration of stereoscopic vision. But it would not therefore be of such a nature as to be in any sense absolute, as if physiological correspondence rested upon a coupling of visual elements in either eye. A certain amount of deviation and latitude would be possible without any prejudice to binocular and stereoscopic vision.

In thus reconstructing the basis of development of the standardisation of the two visual systems, we must evidently avoid the perils of two extremes: on the one hand the assumption of the correlation of the points of the two systems; on the other hand the assumption of the exact equality of the distances evoked in the two systems by one and the same objective distance. The perils of the former extreme we have seen; the perils of the latter are evident in the actual errors of standardisation found in the various radii of either system, as illustrated by the horizontal-vertical illusion. But we must not let the fine accuracy of the binocular gauge of disparity drive us back towards the assumption of an original correspondence of points. If no basis of identification is to be looked for at all in mere points, in distances we may still admit some considerable measure of original bisystemic equality in relation to one and the same objective distance; the deviations we find in one and the same system are quite small, although doubtless considerable from the point of view of disparation, especially as between vertical and horizontal. Moreover, for bisystemic correspondence we do not need anything more than an approximation towards bisystemic equality in relation to one and the same objective distance, so long as we presuppose that each visual system is separately standardised (horizontally at least) in relation to objective distances. The regular bisystemic equality or proportion of forms and changes of forms would then suffice for binocular integration. This standardisation of each eye we may refer to the identity of a given form as it passes over the field of vision, the comparison of a distance as present before and after an eye movement, etc. The presence of a horizontal vertical illusion would then be a natural consequence of our inability to revolve the eye on its axis for the required comparison and of the comparative rarity of rotary movements of objects in nature. At the same time in view of the psychical 'necessity' of stereoscopy (as Kant might have said), we must also admit the operation of the general rule of biological selection in the attainment of bisystemic standardisation. Those whose eyes yielded irregular distance-values for one and the same objective distance would have a poor efficiency of vision, especially if it were so irregular as to put great obstacles in the way of the correction of these irregularities through the medium of form. This problem of psychical standardisation is of great general importance.

(*d*) The correspondence provided by parallel bisystemic variations of acuity of vision would not be of such a nature as to be absolutely unalterable[1], although its advantages and its oculo-motor connexions could never be entirely superseded even in the lifetime of an individual. The appeal to the mediation of form in binocular vision does not imply that the normal correspondence is acquired wholly by the individual. It might nevertheless be dependent for its completion, and therefore also for its maintenance, upon the efforts, or mental activity, of the individual. Thus a person with central scotoma will still fixate with the foveae, although he knows that he sees better if he looks past the object[2]. Any alteration of correspondence would, of course, be very difficult, because it would have to be established in spite of, and against, the disposition of the natural, inborn basis of correspondence, the variations of visual acuity and sensitivity from the foveae towards the periphery. In a new correspondence clearly defined forms in the clearest part of one optical system have to be identified with unclearly defined and larger forms in the peripheral parts of the other system. The impressions of the squinting eye are found to be less valuable—less clear and sharp, less insistent, and less effective in rivalry. This value is regionally different. It is least at the part of the squinting eye that corresponds to the fovea of the other eye and greatest at the fovea of the squinting eye[1]. It is little wonder, then, that in cases of squint although a new correspondence does often arise, it tends to be successive in nature, the impressions of each eye being favoured when clearest; it is also practically devoid of stereoscopic vision, except in the rarest cases, when a rough fragment is secured. For with the disturbance of the coordination of acuity of vision, the identification of similar forms and the integration of disparate forms are made very much more difficult. Still, as the coordination of acuity of vision is not the essential, but only a highly favourable, condition of binocular unity and stereoscopic vision, both of these processes should be possible through the identification and integration of forms in spite of the disturbance of the usual coordination of acuity of vision.

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[1] Cf. Hofmann, *op. cit.* esp. 818 f.

[2] *Op. cit.* 806.

(*e*) The new relations of correspondence established in squinting are not based upon rigidly fixed, bisystemic relations, any more than they are in the normal eye, but they fluctuate and are partly dependent upon the nature of the retinal impressions[2]. This is quite natural. For if the distance between the points shown or attended to is nearly such as would make the one fall on the fovea of the leading eye and the other fall on the fovea of the squinting eye, each point will naturally tend to be brought more precisely to the fovea. Thus there might be a change in the angle of squint without any accompanying change in the 'localisation' of the points falling on either fovea; these points with their new correspondence would be held to be at a certain distance apart—the distance of the average deviation caused by the squint[3]. The angle of squint, as represented in consciousness by muscular sensations, is not the only, or even the important, index of the distance between the points falling on the foveae. The forms of the objects depicted on and around each fovea and on the parts of the unitary visual system between them are of much greater importance. It is quite clear that even although the angle of squint were modified from moment to moment, that change would not suffice to upset the determinations to localisation given by the visual forms presented, once a fairly regular rule of correspondence had been acquired.

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[1] *Op. cit.* 843.

[2] Cf. *op. cit.* 844.

[3] Cf. *op. cit.* 839.

(*f*) Hofmann says: "I do not think it has been satisfactorily explained why, when the attention is turned upon the excentric image of an object in the leading eye, the image of the same object on the macula of the squinting eye should also cross the threshold of consciousness. So long as no abnormal retinal relations have been formed, the two stand in no sort of connexion with one another, unless we assume as a general rule that similar and identical images, situated at different parts of the field of vision, raise each other into consciousness[1]." But is not this mutual support perfectly normal, in the binocular field at least? If one reproduces the state of squint without new correspondence by fixating a near point which is in line with a different object for each eye, does not the mere change of fixation from the near point to one of the farther objects imply that in each eye or in the total field of vision the attention passes to the similar and rival images which "stand in no sort of connexion with one another," and so procures the passage of each image to its fovea? This process cannot be governed by one eye alone; for the distances being disparate, or, as in movements of convergence and divergence, so often of opposite directions, the two images would never reach the foveae together in one act, but only in a series of at least two disconnected acts. But there is no evidence for this serial process, so that the attention must pass to the object of fixation in each visual system independently and previously to the fixation itself. The mere attention to the new object across the disparate distances automatically produces the proper oculo-motor adjustment. But even although the oculo-motor adjustment is complex and the attention passes to an object represented by double images, must we not suppose that the act of attention to this object is single and unitary? I know of no evidence to the contrary. That seems to be the chief reason for our regular neglect of the double images which fill the field of vision. We never do ordinarily attend to the doubleness, but only to the object thus twice represented. At the same time this single act of attention to a new object procures a clearer presentation of the distance or distances which separate the new object from the previous one. If they are identical, their fusion will be immediate; if they are disparate, their disparity will be the more effective according to its capacity. Hence the great importance of change of attention and of the consequent change of convergence for the occurrence of good stereoscopy. For the same reason it is evident that attention does not create or impart depth values; it only provides for the clearest presentation of disparity, which then determines depth values[1].

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[1] *Op. cit* 814.

All this is obviously independent of the question regarding the distinguishability of the two visual systems and of their contents. The two systems may be introspectively distinguishable and really different, and their contents may be effectively uninterchangeable, while their differences are yet in certain respects entirely irrelevant.

(*g*) Only on theoretical lines such as these can we give a plausible account of the compatibility of numerous double images with unitary binocular vision. In this respect our normal vision has a close affinity with the abnormal vision of the squinter. The latter differs from the former only by the fact that the squinter's point of fixation falls almost always by a certain distance either short of or beyond the object looked at by the leading eye, while for normal vision the point of fixation is the object looked at by both eyes. Normal eyes obtain single vision of objects in the plane of fixation and within the range of stereoscopic fusion. For the rest their vision consists of double images, dispersed regularly over the binocular field. This, in the case of the squinter, originally contains nothing but double images. When normal eyes pass from the fixation of a near object to that of a far object or conversely, they work with double images securely, bringing them speedily to their respective maculae and to fusion. The eyes of the squinter also pass through the same sort of distances, but only one image reaches its macula and single vision does not occur. But that is surely no reason for assuming that the squinter's double images do not, in principle, afford him as good a guidance in his passage from double images to double images, as our double images afford us in our passage from double images to single vision. The squinter must attend not to the extra and useless eccentricity given to the images from the squinting eye, but to the distance between the double images from which, and the double images to which, he passes. What can be more natural, then, than that this useless and constant difference of localisation caused by the squint tends to be ignored, since all the distances involved in the forms of objects and in the relative distances between objects are the same as for normal eyes and are the only practical object of attention? A new correspondence would thus be induced by the same means as we have supposed to account for the origin of binocular fusion. But in neither case are double images eliminated; they are only reduced to a minimal amount or to minimal effectiveness.

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[1] This may still allow the experienced attention to impress its mark or decision upon presentations in which depth values are otherwise left ambiguous.

This argument is not at all affected by the fact that the binocular vision the squinter may ultimately possess may not be the product of two simultaneous impressions, but of two successive impressions, one from each eye. Such a process, of course, involves a new correspondence just as much as does simultaneous binocular fusion[1]. It is, indeed, quite conceivable that a new correspondence might arise, although there never had been any really simultaneous vision. In fact, all correlations between experiences, as all integrations that do not involve any differences in the temporal aspects of the integrating components, should be possible both on simultaneous and on successive foundations. But owing to the loss of the usual correlation of the retinae in respect of clearness of vision, and owing to the changes in the angle of squint which often take place in youth, whereby all the labour spent in obtaining a new correspondence goes for nothing, it may well be a fact that cases in which squinters can bring double images to fusion by eye movements, when the degree of disparation of the impressions does not roughly correspond to the angle of squint, are extremely rare[2].

Double images are therefore inevitable in any bisystemic vision dealing with objects at different distances. But unless the accessory physiological adaptations of binocular vision have been disturbed, as in the case of the squinter, their duplication passes unnoticed in virtue of the unity of the movement of attention from the fused image of the (single) object of present regard to the two images of the (also single) object of its next regard. Only when these double images stand in the line of direct attention, as when we look at one object through, or just behind, another, do they stand out clearly. And even then they are commonly ignored because of the regular unity of attention and the movement of fixation which so naturally and so quickly follows a change of attention. To see double images the eye must be fixed and the attention must be directed upon the double images *as double*.

(*h*) It is under these circumstances and others which favour the division of attention that the phenomena of alternation and rivalry appear. There is no rivalry in the foveae in most cases of unitary vision; nor is there rivalry of images near the centre of vision when one of two objects at different distances is fixated. Rivalry is most pronounced when different views fill the uniocular fields; sometimes one predominates entirely, sometimes the other, sometimes the binocular field is a conglomeration of both; anything that gives an extra effective value to one field, *e.g.* a movement of it or in it, immediately draws the attention to it and makes it predominate. Circumstances that would otherwise favour rivalry, *e.g.* differences of colour or brightness, can be reconciled, if a sufficient ground of binocular unity is given, *e.g.* identity of contours or stereoscopic disparity. Rivalry in short is an artifact; either it is the outcome of the frank intention to consider the double images of one object as double and separate, and to resist the impulse to unify them; or it is the outcome of such great differences of images as make this intention natural and inevitable.

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[1] Cf Hofmann, *op. cit.* 814.

[2] Cf. *op. cit.* 833.

This conclusion may be supported by negative evidence. Rivalry cannot be a consequence of any mere periodicity of vision. For we find that both binocular correspondence and stereoscopic vision are possible under conditions of successive integration. In many squinters we find only successive binocular correspondence, and in uniocular vision we find successive stereoscopy. Rivalry is rather a consequence of the incapacity of presentations for forming a single integrative unity. Without such integration they can be apprehended neither in one simultaneous synthesis nor in a continuous successive act, but only discontinuously and separately.

VI. *The role of form in the process of uniocular stereoscopy.*

If the right and left eye pictures of a landscape or of a group of familiar objects are projected alternately upon the same screen in a suitable adjustment to one another, a good stereoscopic view is obtained. This view certainly oscillates in the line of regard in a disagreeable manner, but there can be no doubt of the plasticity of the result. But if right and left eye views of a stereoscopic complex of points are presented in the same manner, no plasticity emerges. The points simply oscillate from the position they occupy in the one picture to the position they occupy in the other, as if they were themselves in simple motion. An integration, then, does result, but it is the lowest integration that can emerge from the presentation of points at different places in successive moments—the integration of simple motion[1].

Now it might be argued that, because solidity does not emerge from the combination of complexes of points but is obtained from the combination of pictures, solidity is somehow already present in the latter. But this conclusion is neither helpful nor necessary. It offers to explain the great difference between the effect of the pictures and the effect of the groups of points by positing in the single pictures the presence of a minor degree of that same difference without explaining why the oscillating double presentation should so heighten the degree of difference. The single picture certainly does not seem to be stereoscopic, although it obviously represents a solid scene; whereas the effect of the combined pictures is undoubtedly stereoscopic and that of the complexes of points is certainly not so. But difference of a lower order does exist between the pictures and the points, a difference of form. The picture presents a relatively unambiguous and unconvertible mass of forms, while the complex of points can be moulded at will into all sorts of masses of forms. If the combined effect of the oscillating views is to get beyond the minimal integration of motion, each single exposition must ensure the integration of its constituent points into stable forms. As the difference between the two cases does not lie in disparity of constituent points but in stability of form, we may well infer that stereoscopy is primarily an integration of forms. We thus confirm our previous conclusions.

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[1] Cf. This *Journal*, 1913, VI. 26 ff.

Within certain natural limits variation of the temporal relations of the successively presented pictures makes no difference whatever to the resulting stereoscopy. The rate of alternation must not be so slow as to jeopardise the integration, nor must it be so fast as to lead to the practically simultaneous presentation of the two disparate pictures. In the latter case the after-images of the two pictures overlap and confuse one another, so that the proper presentation of forms presupposed by stereoscopy becomes impossible. This hindrance does not arise so readily in normal uniocular stereoscopy, when the eye is translated over against objects. Then the integrating forms are progressively disparate and form a continuous series. But after-images and confusion will, of course, appear when a certain rate of translation is exceeded. The independence of the stereoscopic integration stands in marked contrast to the way in which the motion which always accompanies progressive uniocular stereoscopy is affected by the rate of presentation of successive views. The speed of motion is, of course, dependent on that rate. Thus whereas motion is an integration of progressively different orders at different times, stereoscopy is an integration of disparate forms that is independent of difference of times. Stereoscopy is independent of the temporal aspects of the experiences which integrate to form it[1]; it cannot be got from mere orders; it must be based upon disparate complexes of orders or forms.

VII. *Theory of stereoscopic vision.*

(*a*) *The rules of bisystemic disparity.* The relations between the systemic components of binocular stereoscopic vision or between the successive contents of a single system in uniocular stereoscopic vision can be stated in the form of simple rules. It is the custom to state such rules in relation to the stimulations of the retina, as if these were somehow actually projected into space. They will be stated here apart from all relations to the retina and with regard only to the differences between the experiences which enter into and constitute stereoscopy. The rules are as follows:—

Binocular. (1) If the distance between any two points is greater in the right system, the right of the two points which bound that distance seems farther away. (2) If the distance....is greater in the left system, the left of the two points....seems farther away. (3) The case of disparity of distance of opposite signs is a special one, as double images are then much more pronounced. But it may be considered as falling under the above rules, if opposite signs, plus and minus, are introduced, or if both rules are taken to hold simultaneously, each deviation of one of the two double images, away from the other side of the point fixated, being, as it were, the nil distance for the positive distance between the point fixated and the other double image.

Uniocular. Here the right and left systems of binocular vision are replaced by a series of disparately changing contents of one system. We are not free to suppose either direction of transference as in binocular vision, from right to left system or vice versa. The direction of change is fixed and may be taken as always the same, that is to say, a forward direction, no matter whether an animal moves really forwards or backwards, or upwards or downwards. But if we wish to have both positive and negative forms for our rule, we may suppose that the eye alters its fixation, attending in one case to the backward of the two points that bound a distance, in the other case to the forward point. In the former case the forward point is further away, if the distance between it and the point fixated increases in successive moments. If the forward point is fixated and the distance between it and the backward point increases, the latter is nearer, according to the rate of increase of the distance in successive moments. Putting both statements together, we may say: if the distance increases forwards (in the line of progression), the forward point is farther away. The opposite is: if the distance decreases forwards, the forward point is nearer.

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[1] Except in so far as these determine the development of disparation, as in uniocular progressive stereoscopy.

General rule. We may consider transference from left to right system or vice versa and a fixed sequence of experiences as variant forms of the same process. We then obtain a single general rule for all cases: if a distance increases in the direction of systemic progression (left to right, or right to left, or forwards, or backwards, etc.), the forward (right, left, forward, upper, etc.) bounding point is farther away; if a distance decreases in the line of systemic progression, the forward bounding point is nearer.

This rule implies no successive treatment of these increases or decreases, not even in the case of uniocular stereoscopy. Nor does systemic progression, *e.g.* from the left to the right system, imply a parallel passage of the attention from the left to the right bounding point of a distance. These systemic transferences are only supposed for the purpose of stating the relations between the integrating components. In reality no movement of the attention is required, except in so far as the benefits of fixation and of use of the point of clearest vision and the necessity for keeping up the integration of the component forms make a movement and distribution of the attention necessary.

In considering the rule we have also to remember that we are not dealing with a mere increase of distance as such, but with the simultaneous apprehension, in the case of binocular vision, of one distance along with a greater one, and in the case of uniocular vision, of a distance along with its progressive increasing. We are dealing with integration, not with mere observation of change. The integrating parts unite to form a new complex. Thus we can substitute for the traditional and easy reference to points in the rules formulated above a reference to the true basis of stereoscopy in disparity of distances and to the true nature of stereoscopy as a kind of distance. Increase of distance means, then, an addition to distance; something more than mere 'plane' distance, as it is before it enters into integration; a special kind or modification of distance. Decrease of distance means a deduction from the ordinary magnitude of distance, a drawing in or restriction of distance. If we go beyond the scope of the elementary mode of distance and use the more complex and general term 'form,' the result is similar. Bisystemic, or successively monosystemic, increase of the proportions of form gives a special extension and expansion to form; decrease draws in and contracts forms in a special way.

(*b*) *The negative case of 'vertical' disparity.* The above statement of the rules for lateral disparity implies nothing more than the actual differences of the 'plane' visual experiences given and the distinction between the systems of binocular integration and the successive monosystemic complexes of uniocular integration. The most striking features of the rules educed is their symmetry: left system increase over right system, left point farther away than right, and vice versa; if a distance increases towards any system or in any line of systemic progression, that distance then shows a peculiar kind of increase ('away') in the direction of that system.

It is a crucial difficulty in every theory of stereoscopic vision to show why that vision is not possible under conditions of vertical disparation, whereby by 'vertical' is to be understood 'perpendicular to the line joining the two eyes or to the real direction of systemic progression.' Of course, the fact that under present conditions of reality we could not possibly receive from any complex of natural objects impressions in the two eyes or in one moving eye which differed in their 'vertical' dimensions, is no reason for the absence of stereoscopy from vertical disparation, as it is artificially presented in the stereoscope. Custom is no excuse or explanation for the automatic operations of sense. Nor does the failure of vertical disparation result from the firm coordination of the two eyes to parallel movement in the vertical direction. For that coordination has been born of circumstances, just as has the ease of disparate movement in the horizontal dimension. The products and needs of vision have led the way, not the exigencies of muscular control. Besides, stereoscopy is possible in its uniocular form under any direction of monosystemic progression, forwards or vertical, so that there is nothing specially repellent about vertical disparity as such. Our two eyes might well have been posted the one vertically over the other in our heads, had it not been that the needs of terrestrial life have made it necessary for all our sense-organs to be placed so as to give the greatest scope and sensitivity in the horizontal direction, parallel to the earth's surface. Our eyelids are slit horizontally, the cat's pupils are slit vertically, our eyes are placed on a level, and so are our ears, all for this same purpose.

The true reason for the failure of vertical disparity seems to be that we cannot obtain from it any such symmetry as we obtain for lateral disparity. What would decide between the two cases? We achieve only chaos if we try to convert the general rule for lateral disparity into one for vertical disparity. If the vertical distance increases in the left system, why should we then expect any decision as to which of the two points that bound the distance perpendicular to the direction of systemic progression is farther away? A special increasing or decreasing of distance can mean nothing for experience under these circumstances. It seems natural and inevitable that a plus of distance in the left system should mean a plus of distance to be traversed to get to the left bounding point of a distance, a kind of plus that is new and is not given or expressed by either of the integrating distances. And it seems natural that a decrease towards the right system should bring the right bounding point nearer, should lead to a special modification of the distance[1] between it and the left point.

Of course I do not mean to attempt with these words a rationalisation of the real; for the real that is not itself of thought, cannot as such be turned into thought and made conceptually necessary. With realities, even psychical realities such as the primary processes of sense, we are limited in our science to complete description, arrangement according to similarity and dependence, and to systematisation of all relations; and we expect that satisfactory systematisation will be the inevitable consequence of sufficient description and arrangement. There is no doubt in my mind that the rules given for lateral disparity are systematic and regular in a way that no rule for vertical disparity can be. There is a self-determination about the former that is not found in the latter. And that seems to me a sufficient 'reason' for the presence of stereoscopy under conditions of lateral disparity and its absence under those of vertical disparity. For in the latter case no regularity or natural grouping is provided which would form a basis for a further step in integration. More than that can hardly be required as an explanation of the presence of stereoscopy under lateral conditions.

(c) *The systemic sign.* The rules for disparation given above imply a distinction between the integrating components. In uniocular stereoscopy this distinction is given by the perceptual stability and unambiguity of the form-mass of each instant[2]. But in binocular stereoscopy no complexity of form is required; a perfectly stable result is obtained from the fundamental stereoscopic slide of two points. This superiority is due to two things: for the one, binocular stereoscopy is devoid of the disturbing effect of motion, which always accompanies the uniocular integration, and which so strongly favours the analytic observation of points as against the comprehensive observation of forms; for the other, there are involved in binocular stereoscopy two distinct visual systems. In the preceding paragraph we have just seen how indispensable this distinction of systems is.

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[1] A modification of the specific variable of distance—its magnitude—by special circumstances is familiar in the Muller-Lyer illusion. An alteration of the orders of its bounding points is not the antecedent, or even a necessary consequence, of the primary change of distance magnitude, but depends upon an attitude of observation.

[2] Cf. VI above

The distinction of systems is also an obvious implication of the striking reversal of all depth relations which follows when the right and left eye pictures of a solid geometrical figure are interchanged. But if the pictures of a landscape are interchanged in this way, the same striking reversal does not occur; at the most stereoscopy lapses entirely and the reversal appears only at one or two more formal and less familiar outlines in the picture. Were the whole picture reduced sufficiently to its formal components, reversal would doubtless be observable in every part. But in the intact picture reversal cannot proceed far, because the fragments of form which it offers fail to harmonise with our memory of the forms of natural objects. Thus memory not only gives no guide to the survey of the complex of reversed forms, but it also disturbs and misleads it, so that the integration dissevers into its plane components or becomes a useless medley of their parts. This necessity for method and guidance in stereoscopic vision we shall consider later in more detail. For the reversal of simple geometrical figures the habits of past experience are sufficient. And to a certain extent, of course, the stereoscopic process is itself autonomous; for it is the primary basis from which experience accumulates its habits and expectations[1].

The failure of reversal in the case of landscapes and such views, therefore, in no way weakens the necessity for postulating a distinction of visual systems. In fact it itself presupposes that distinction. For it is alone the cause of the unwonted constitution of the combined complexes of forms which baffles the experience of the observer. Binocular stereoscopic vision thus in all its variations presupposes a distinction of systems. What is the nature of this systemic distinction?

It was suggested above (§ V. (*a*)) that no original correspondence of the eyes could be assumed; it must have developed out of an original independence; and the natural progressive medium of this process seems necessarily to be the identity and similarity of the forms and changes of forms that present themselves in the two systems. It is in keeping with this conclusion to suppose that the systemic sign is not a peculiar attribute, unrelated to the simple attributes of sensation, a sort of heaven-born mark for the purpose and convenience of the bisystemic integration which in the course of time should be achieved. A sensory system is constituted by the differences of order which attach to the sensations evoked by different elementary receptors. We can expect in different systems, therefore, only that same kind of difference, namely differences of order. Two questions then emerge: first, does this difference of order occur, and second, can it fulfil the functions of the systemic sign?

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[1] Hence a great variety of relations are possible between the primary autonomous basis of stereoscopy and all the attitudes and expectations which have grown upon our experiences of the corporeality of objects. Cf. L. v. Karpinska, *Ztsch. f. Psychol.* 1910, LVII. 55 ff.

1. A difference of order between the two visual fields is implied in the correlations into which these fields enter with the space of our sensory experience in general and with conceptual space. Binocular vision is said to be Cyclopean; the clearest point of the binocular field of observation virtually occupies a position midway between the two eyes. But the virtual position of the clearest points of each system considered singly and compared with one another do not coincide with this. The clearest point of each system virtually lies in the centre of each eye. This difference presupposes an original difference between the orders of the two clearest points and correspondingly of all the other points of the two systems. For if correspondence implied a radical identity between points in either system, such a difference of virtual position could not possibly arise. Even the old appeal to muscular associates is in this case made impossible primarily by the fact that its task would be to make 'two' identicals different by associating them with differences[1].

But if we admit an original difference between the two visual systems, an appeal to associates becomes admissible. And the explanation of virtual functions seems to require this appeal. For the virtue is indeed more than visual; it is the position of this or that *object*; it is an order of a supersensuous system, just as is the constancy of localisation of an object observed momentarily with either eye and both eyes alternately. Such serial observation does not isolate the ordinal functions of either eye from the complex processes of localisation which give objects an 'absolute' localisation, but rather serves to preserve the latter throughout. It may indeed be doubted whether the localising processes built upon vision can ever be sufficiently severed from vision itself so as to lay bare to direct observation the primary ordinal differences between (at least) the 'overlapping' parts of the

[1] Cf. I. (*d*) above. This *Journal*, 1913, VI. p. 248.

two visual systems, differences which effectually permeate binocular integration and the localising processes built upon it[1].

But an original difference of psychical orders would not make their identification for certain purposes impossible; as if the Cyclopean position of observation were a 'real' position that had somehow to be constructed out of two other 'real' positions or to suppress them and take over and, as it were, average their functions. In view of the peculiar nature of these functions which no sort of averaging suffices to formulate, we must assume that binocular 'identification' is superimposed upon difference without any prejudice to the continuance (though with much prejudice to the observability) of that difference. Such a method of identification calls for a means of identification such as we have found in identity and similarity of form. But it is only natural that the view-point of the united system, in which two large parts of each system are brought to identification round the area of clearest vision, should be central to the whole united system.

The two eyes, then, originally formed homogeneous parts of one simple system. They were distinguished, not by any peculiar systemic sign, but only by their orders. Now their orders have been for a certain range and through the medium of common forms integrated to form a new system, but their original differences are still present and operative within that integration and others built upon it. In the rest of the range of each system the original ordinal differences are effective in the original way, *i.e.* each uniocular part of the total field of vision is simply a special part of the total binocular system, just as the skin and underlying tissues about the two shoulders are special parts of the total and homogeneous tactual system.

It follows from this reconstruction of correspondence that it is in all respects functionally valid as a contributor to the new series of orders given by stereoscopy. It is in fact, as it is functionally, a special case of stereoscopy and of the new system of orders established as a secondary result of the integration of identical or similar forms presented in the two visual systems.

2. Nor can we doubt that this original difference between the two systems can fulfil the functions we have ascribed to the systemic sign.

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[1] This also explains the very negative or indecisive results of the examination of the distinguishability of the two visual systems. But these results of course do not preclude their being effectively distinguished, or being distinguishable under favourable circumstances, especially on the basis of the eye's localisation. At the same time one might very well know that one eye had been occluded without knowing which, except under circumstances favouring localisation.

If the systemic sign were a new attribute unallied to the attributes of elementary sensation, the same difficulty of decision would arise in all cases of binocular vision as faced us above in the discussion of 'vertical' disparity. We should be unable to see any rationality or systematic necessity in a coalition of the systemic sign (of non-ordinal nature) of the one (left) eye with that modification of the leftwards direction of a distance as further away which should accrue because the distance in the system of that same systemic sign had increased. The peculiar systemic sign and the leftwards direction of the distance would have no psychical kinship. Stereoscopy would then be as likely with vertical as with lateral disparity. But as we have construed the matter, the leftwards order that constitutes the systemic sign of the left system and the leftwards direction of the distance that occurs in disparate magnitudes, have as natural an affinity for one another as the increase of the distance in the left system has with the greater 'awayness' from the observer of the left end of the integrated stereoscopic distance. Apart from the real integrative step there is, therefore, in the binocular stereoscopic process nothing mysterious or unsystematic.

The systemic sign of binocular vision has, of course, a counterpart in uniocular vision. But the problem is here very much simpler apart altogether from the guidance afforded by the solution of the binocular difficulty. Uniocular stereoscopy presupposes an awareness of the direction of systemic progression, or of the translation of the eye. No special sense is required for this; for the translation of the eye and the systemic progression are always forwards, whatever the real direction of the motion of the body may be; so the direction of systemic progression can be given quite well by the flow of experience through the visual system. When a distance increases, then, the point lying towards the 'entrance' of the visual field is farther away. Of course, the direction of the flow of visual experience will inevitably enter into associative correlation with the other sensory activities of the moving animal.

It may be recalled[1] in passing that an awareness of the rate of systemic progression is also presupposed by a fully perfected experience dealing with the distances of objects from the body on the basis of uniocular stereoscopy. But for relative and purely visual stereoscopy no such awareness of the rate of progression is required. Awareness of the rate of progression[1] certainly cannot be visual; it must come from some other special sense. And the experiences into which with vision it enters, to give a fully perfected experience of distances from the body, lie beyond the scope of the specifically visual integration of stereoscopy.

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[1] Cf. above, IV (*d*).

(*d*) Stereoscopic vision may, then, be collectively described as *an integrative complex of systemic 'forms,'* given either simultaneously in two different systems or successively in one and the same system. The complexity within each system in human vision at least is very great, and special means are necessary in order to facilitate the proper coordination of the forms of the two systems, so that they can be traversed at will, and brought into such correlation with one another at any moment as will make integration rapidly possible. These special means are accommodation and convergence. Their nature and manner of operation are well known and need not be recounted here. But it is a mistake to look upon them as in any way responsible for the stereoscopic integration or as themselves the source or basis of stereoscopy. They facilitate the presentation of such forms as will integrate to stereoscopy, but even in this respect they are the servants and instruments of vision, being controlled by vision for its own purposes. They are the means by which vision so controls the presentation of visual data that it can always maintain its highest integrative efficiency.

At any one moment a block of stereoscopy is given which stretches out in either direction in the line of sight from the points fixated as far as the range of the integration of disparates allows. By a suitable change of fixation this block may be extended in a perfectly continuous manner in either direction towards or away from the observer. Thus the two systems of forms provided by the use of two eyes are continually wrought into one another in a systematic way.

In other words stereoscopy is primarily the production of a third direction or dimension of form. Form, be it noted, not space! The orders of vision, as of all the senses, doubtless stand in objective correlation with variations in the spatial positions of elementary receptors; they are really correlated with spatial positions and extents. This real correlation is the basis of all else that may follow. But that does not necessarily include at any point a knowledge of spatial positions and extents. In the sense of sound, for example, orders and extents are given as in other senses and they integrate intrasystemically, as in other senses; but these integrations form no basis for the production of a knowledge of objective spatial relations; nor do they present themselves as spatial in experience[1]. The intrasystemic integrations of sound are in no way inferior to those of any other sense, although they present certain more or less explicable peculiarities of their own[2]. If there is in them no evidence of any conversion of orders and extents into spatial experience, neither can there be any in any other single sense. The experiences of sound are ordinal and extensive, and so are their integrations after their kind. The experiences of other senses, including stereoscopic vision, are the same. Mere order and extensity, therefore, does not make it possible for a sense to become spatial. Nor does the mere presence of higher mental processes. For the orders and volumes of sound coexist in our minds with highly elaborate cognitive processes and resist all attempts to make them the medium of spatial knowledge. Spatial knowledge requires besides for its occurrence the existence of proper and sufficient real correlations between the ordinal variations of sense and the spatial positions of things. And just this is lacking in hearing.

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[1] Awareness of mere change of acceleration would be sufficient in many respects. We know how much more incisive the effect of a change of convergence is than any mere state of convergence. But traces of other absolutising factors are to be found; so we may at this juncture let the absolute implication of the phrase 'rate of progression' pass.

Thus we may now feel more reconciled to the conclusion that the purely visual process of stereoscopy yields tridimensional form, but not tridimensional space. But it is capable of becoming spatial, for it does so, as we know. Every one that is familiar with the history of the study of visual space will recognise the benefit of having the several fields of problems included in it thus clearly separated. The full development of vision involves the cooperation of vision with other senses and a long course of accumulative experience. But all this and much more of the same kind now offers no opposition to the conclusion that stereoscopy is a purely visual and spontaneous integrative process for which no long course of experimental and accumulative experience, in the individual as against the racial sense, is required. All these trials and acquisitions lie beyond the tridimensional vision of form to which the name of stereoscopy has come to be attached specifically.

(*e*) *The interplay of primitive stereoscopy and higher determining processes.* The above theory harmonises perfectly with the masses of observations recently collected by v. Karpinska, Jaensch, and others, regarding the nature of the stereoscopic process.

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[1] Cf. "Psychological Analysis and Theory of Hearing," This *Journal*, 1914, VII. 1 ff., esp. 27 f.

[2] Cf. *op. cit.*

As v. Karpinska[1] shows: stereoscopic (v. Karpinska says: 'spatial') apprehension develops out of the flat; in stereoscopic apprehension the whole complex of forms is much more of a unity than in the flat picture, where each unit of form is more or less independent within the whole (v. Karpinska's figures were geometrical); apart from all eye movements there is a successive process of seizure of the drawings by the attention; and the front parts of the stereoscopic complex are more obtrusive. Stereoscopy is thus an elaboration of the flat 'sensation.'

The forms which are the basis and means of stereoscopic integration are good ground for all, or any, of the wanderings of attention which Jaensch[2] has detected. The fields of theory covered by Jaensch's work and by the present paper neither overlap nor exclude one another; rather are they really complementary, although Jaensch seems to think that stereoscopy might be explained without remainder by the process of wandering attention and the localisation it somehow conveys. But it must be clearly evident to the reader of Jaensch's work that in every explanation by reference to the attention the essential basis of stereoscopy is already presupposed.

These recent introspective works are characterised by a common desire to disparage the importance of the part played by disparation in the stereoscopic process in favour of 'more psychical factors.' v. Karpinska wishes to consider disparation merely as an empirical criterion of space[3]; and Jaensch tries in vain to put it behind his back. The motive of these efforts is no doubt the desire to emphasise the psychical nature of the process of stereoscopy. But that is surely best to be done by 'going the whole hog' boldly and by accepting the principles of a pure psychology even for a study of the senses. Then disparation will also be in action only as a psychical factor. And an end will be made of those amalgams of psychical processes and psychophysical presuppositions which are so easy to make but so useless for permanent constructive work. A single example will serve to recall many: Jaensch *presupposes* in his theory "the fixed position and the sharp distinction of corresponding retinal points[4]." Others are the distinction of the visual systems even within the stereoscopic process, the exclusion of vertical disparity, the existence of a tridimensionality of unknown or even of cerebral origin[1], etc. Until these things are included within the scope of a theory of stereoscopy and solved on such purely psychological lines as have been indicated in this paper, the theory of stereoscopy will never settle down into a form whose main outline could seriously be considered to be final.

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[1] *Ztsch. f. Psychol.* 1910, LVIII. 1 ff.

[2] E. R. Jaensch, "Über die Wahrnehmung des Raumes," *Ztsch. f. Psychol.* 1911, Erg. Bd. VI.

[3] *Op. cit* 47

[4] *Op. cit.* 355.

I have already referred to the conflicts which may arise between the primary psychical factor—disparation of form—and the secondary factors of perspective, shadows, and other special complexes of intrasystemic forms, attitudes, etc. These conflicts afford no evidence that the latter factors are the primary source of stereoscopy. The mere fact that disparation, though 'empirical,' is the ultimately and overwhelmingly decisive force shows that. But special argument about primacy[2] is impossible here; such matters can only be settled on the basis of a broad survey, for which I refer to my series of papers on sensory integration[3]. If disparation of form is the prime source of stereoscopy, the other factors are 'associated' and derivative, as we should indeed from their nature expect them to be. Conflicts are due to forces directed from upper strata of integration upon lower ones under the influence of 'attention.' Attention is an agent that can act only upon instructions or after proper education. It can then mould, or determine the formation of, such unit-groups of experiences as include the operations of recall, but it cannot 'produce' any original integrate out of itself. Where indeed would it take this new integrate from? It is surely futile to try to draw the modes and forces of the lower levels of experience from its upper levels.

Jaensch's neglect of intrasystemic and bisystemic forms becomes evident in the necessity he feels for postulating a central process "to provide in the greatest part of visual space the sensory material for the third dimension[4]." There can be no doubt that this is an attempt —utterly inadequate—to provide for the wanderings of attention with which he hopes to explain the whole stereoscopic process, a sufficient basis of operation. But for the experience of form—and so of stereoscopy—no wandering of the attention is originally required, however much a wandering of the attention may do towards vivifying and extending, as may be needful, the spread of attention and so of stereoscopy. Thus we do not require to add to a process of attention already completely 'wandered' because of the lack of any original unity and coherence or leading amongst its experiential objects a mysterious 'tendency to persist[1]' by which its futile doings might gain substance. We require only to admit the operation of a higher process of attention, whose origin and operations it is the duty of students of higher mental processes to account for, and to allow that this upper force interacts with the forces of the lower levels of experience to aid or to hinder them, and that the area or sphere of its operations is not constant, but changes from moment to moment, while any sort of permanence in its objective calls for its repeated renewal.

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[1] Cf. Jaensch, *op. cit.* 295 ff., 327.

[2] Cf. *op. cit.* 324 f.

[3] *v.* above, p. 133.

[4] *Op. cit.* 304.

It is impossible to enter into a detailed discussion of the introspective findings of these papers. It is indeed unnecessary, as they in no sense offer any opposition to my theory, but only give it the confirmation it admits of, and point to the detailed interaction in experience of the stereoscopic process and all other higher processes of experience, especially those of the 'determining' class. The study of all these connexions on the higher side is undoubtedly of the greatest interest. Yet no psychological theory of stereoscopy can lay claim to any sort of completeness, or can force conviction, that does not include a satisfactory treatment of the various aspects of the problem dealt with in this paper. We may certainly explore from above downwards; but, if the success attaching to theoretical procedure in the natural and biological sciences counts for anything, we must build our theory from below upwards[2]. And it may properly be expected of a theory such as I have here presented, that it will serve as a basis and a guide for the proper arrangement of the facts that lie psychically on the farther side of the purely visual process of stereoscopy.

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[1] Beharrungstendenz, *op. cit.* 327.

[2] I am quite at a loss to understand how "development has proceeded in the sense of a differentiation of rough mass-reactions," if that is taken as equivalent to the comprehension of the 'lower' in terms of the 'higher,' the 'sensation' in terms of the 'integration' (v. Koffka, *Ztsch. f. Psychol.* 1914, LXIX. 117). Of course there is in the soul, as in life and nature, every variety of specialising, reflecting, and isolating, 'reaction'; but all of these are first preceded by the primitive spontaneous 'action' that alone makes masses of all kinds possible. Where we are concerned only with systematisation, we can 'comprehend' the system from any end; but development is surely to be 'comprehended' from its beginning onwards, and any appearance of development from the complex to the simple is surely illusory.

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FACTORS IN THE MENTAL PROCESSES OF SCHOOL CHILDREN[1].

III. FACTORS CONCERNED IN THE SCHOOL SUBJECTS[2].

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IN the paper on "General Ability," by Dr Hart and Professor Spearman[3], a description is given of three theories held as to the relation existing between mental performances. They are, firstly, the 'non-focal' view, according to which the correlations between mental performances are independent; secondly, the 'multifocal,' which assumes a number of 'levels' and a high correlation between performances belonging to the same level; thirdly, the 'unifocal' view, assuming the existence of a general common factor. Further than this, a criterion is given to enable an investigator to determine which of the three theories his observations support. To quote from the paper, p. 59: "If the older view of Thorndike, viz. a general independence of all correlations, holds good, our correlation between columns of correlational coefficients should always average about 0. If his newer view of 'levels,' or the almost universal belief in 'types' is correct, then the mean correlation between the columns should be a low minus value. If, finally, the true theory is that of a General Common Factor, the correlation between the columns should be positive and very high" (*i.e.* close on + 1·00).

After reading this paper, having material at hand which had been collected for other purposes, it occurred to me to treat it in the manner described on p. 55 of that paper. The material used consisted of the examination marks obtained by children ranging in age from 7 to 14 years, in classes II to VII of the senior departments of four London County Council elementary schools. The subjects for which marks were obtained were the following: geography, history, science[4], arithmetic, reading, composition, spelling, writing, painting and needlework.

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[1] Thesis approved for the degree of Doctor of Science in the University of London.

[2] For Section I see this *Journal*, Vol. VII. Pt. 4, 453. For Section II see this *Journal*, Vol. VIII Pt. 1, 70.

[3] "General Ability, its Existence and Nature," this *Journal*, Vol. V. Pt. 1.

[4] In these particular schools the science syllabus comprises very elementary lessons in general science and nature study.

Table I gives the correlations between these ten subjects; the figures in black type are the reliability coefficients. The number of children examined was about 500. However, there are not 500 cases represented in every correlation. In geography, history and science the number of cases is much smaller. Elementary school children are not always examined in these subjects; and the examination and marking are often not suitable for scientific purposes. If the reliability coefficient of the marks for a class in any particular subject fell below ·50, that series of marks was not used. This happened frequently in certain subjects, particularly writing and needlework. This seems to be due to the smaller individual differences in these

TABLE I. *Correlations between the School Subjects* (*'Corrected Coefficients'*).

	Geog.	Sci.	Hist.	Arith.	Comp.	Read.	Spell.	Writ.	Paint.	Need.	
			A			B			D		
Geog.	**·55**	·84 ±·*025*	·88 ±·*018*	·65 ·*031*	·73 ·*027*	·62 ·*034*	·53 ·*049*	·40 ·*062*	·32 ·*055*	·52 ·*053*	
Sci.	·84	**·60**	·75 ·*035*	·63 ·*053*	·69 ·*046*	·57 ·*060*	·43 ·*085*	·34 ·*105*	·44 ·*060*	·48 ·*075*	
Hist.	·88	·75	**·56**	·67 ·*040*	·68 ·*036*	·66 ·*039*	·44 ·*077*	·27 ·*075*	·17 ·*092*	·22 ·*096*	
Arith.	·65	·63	·67	**·69**	·62 ·*024*	·46 ·*026*	·52 ·*031*	·39 ·*034*	·41 ·*036*	·37 ·*041*	
Comp.	·73	·69	·68	·62	**·73**	·64 ·*021*	·75 ·*018*	·48 ·*032*	·42 ·*035*	·27 ·*040*	
						C					
Read.	·62	·57	·66	·46	·64	**·74**	·79 ·*020*	·36 ·*035*	·28 ·*035*	·12 ·*041*	E
Spell.	·53	·43	·44	·52	·75	·79	**·74**	·40 ·*038*	·24 ·*046*	·17 ·*050*	
Writ.	·40	·34	·27	·39	·48	·36	·40	**·70**	·52 ·*035*	·54 ·*033*	
Paint.	·32	·44	·17	·41	·42	·28	·24	·52	**·72**	·52 ·*036*	F
Need.	·52	·48	·22	·37	·27	·12	·17	·54	·52	**·67**	

Figures in black type = reliability coefficients.
Group *A* = intercorrelations of geography, science, history and arithmetic.
„ *C* = „ „ composition, reading and spelling.
„ *F* = „ „ writing, painting and needlework.

subjects, in which case it is always more difficult to obtain a satisfactory reliability. The actual number of cases for each correlation can be estimated from the size of the probable error in conjunction with the reliability coefficients[1].

TABLE II. *Correlations between columns of correlations of Table I.*

	Geog.	Sci.	Hist.	Arith.	Comp	Read.	Spell.	Writ.	Paint.	Need.
Geog.		·98 *·76*	·91 *·91*	·90 *·78*	·58 *·63*	·70 *·57*	·43 *·55*	— *−·50*	−·60 *−·18*	−·53 *−·15*
Sci.	·98 *·76*		·96 *·83*	·85 *·83*	·69 *·50*	·42 *·47*	·51 *·60*	−·84 *−·32*	−·69 *−·27*	−·30 *·00*
Hist.	·91 *·91*	·96 *·83*		·94 *·77*	·80 *·75*	·68 *·55*	·73 *·73*	— *−·28*	−·38 *−·16*	−·51 *·08*
Arith.	·90 *·78*	·85 *·83*	·94 *·77*		·74 *·78*	·76 *·68*	·32 *·62*	— *−·13*	−·85 *−·29*	−·32 *−·18*
Comp.	·58 *·63*	·69 *·50*	·80 *·75*	·74 *·78*		1·00 *·89*	·75 *·81*	— *−·40*	−1·06 *−·54*	−·71 *−·46*
Read.	·70 *·57*	·42 *·47*	·68 *·55*	·76 *·68*	1·00 *·89*		1·27 *·79*	— *−·41*	−·96 *−·54*	−1·14 *−·54*
Spell.	·43 *·55*	·51 *·60*	·73 *·73*	·32 *·62*	·75 *·81*	1·27 *·79*		— −·18	−·63 *−·40*	−·82 *−·65*
Writ.	— *−·50*	−·84 *−·32*	— *−·28*	— *−·13*	— *−·40*	— *−·41*	— *−·18*		— *·39*	— *·31*
Paint.	−·60 *−·18*	−·69 *−·27*	−·38 *−·16*	−·85 *−·29*	−1·06 *−·54*	−·96 *−·54*	−·63 *−·40*	— *·39*		·93 *·64*
Need.	−·53 *−·15*	−·30 *·00*	−·51 *−·08*	−·32 *−·18*	−·71 *−·46*	−1·14 *−·54*	−·82 *−·65*	— *·31*	·93 *·64*	

The usual procedure was to take the marks obtained in two consecutive term examinations for each subject. The correlation between these two sets gave the reliability coefficients. In classes where weekly or monthly test marks were kept, these were used and two sets of marks were obtained by adding up the marks for alternate weeks or months. Then the 'corrected' correlations between the ten subjects were found

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[1] By a known formula

$$\text{p.e.} = \cdot 6745\, \frac{1 - r^2_{x(p)\,y(q)}}{\sqrt{n}} \cdot \frac{r_{xy}}{r_{x(p)\,y(q)}},$$

(see this *Journal*, 1910, III. 294) from which, when p.e. is given, it is easy to calculate n.

for each class. Finally, the corresponding correlations in all the tables thus obtained were averaged and the results are given in Table I[1]. (The division of the table into sections will be explained later on.) This table was then treated in the manner described on p. 55 of the paper on "General Ability." All the correlations between the different columns of correlations were found, and the results tabulated as shown in Table II. These correlations, where they conformed to the standard laid down in "General Ability," p. 56, were corrected. It was found that all the correlations were corrigible except those involving the subject 'writing'; the uncorrected figures are given below the corrected.

On examining Table II, one is immediately struck by the surprisingly regular groups of plus and minus correlations into which it falls. But it does not appear to correspond with any of the theories stated in "General Ability." These three theories deal with the simple cases mentioned above. By the first, every correlation is independent; the second assumes specific factors each common to a certain group of performances; the third, the general factor common to all performances.

TABLE III.

	1	2	3	4	5	6
1		$a+p$	$b+p$	c	d	e
2	$a+p$		$c+p$	d	e	f
3	$b+p$	$c+p$		e	f	g
4	c	d	e		$q+q$	$h+q$
5	d	e	f	$g+q$		$i+q$
6	e	f	g	$h+q$	$i+q$	

Let us consider what would happen in the case of a complication of theories 2 and 3; that is, how a table showing a hierarchical arrangement owing to the presence of the general factor would be affected by the presence of specific factors occurring in certain groups of performances. Suppose all the correlations in Table III to derive solely from one general factor. Then $a, b, c, \ldots i$ (p and q will be explained later), the correlations between performances 1, 2, ... 6, may be taken as decreasing in order of magnitude, and for simplicity we will suppose the decrease to be of a regular character. The correlations between the rows and columns will be positive and approximate to unity. For instance, columns 1 and 2 will be as shown in Fig. 1. Next suppose a specific factor to be involved in performances 1, 2 and 3. This will increase all the inter-correlations between these performances. Suppose this increase to be about the same amount in each case and let it be denoted by p. Whether the mean variation between correlations forming a column be great or small, it is easy to see that the specific correlation p will have very little influence on the correlation between these two columns, as the correlation $a + p$ in column 1 will still be higher than c, d, and e in column 1, and $c + p$ higher than d, e, and f in column 2. The same will be true of the correlations between columns 1 and 3, and 2 and 3. Even the correlations of columns 1, 2 and 3 with 4, 5 and 6 (Fig. 3) will be little affected for the same reason, namely, that corresponding terms still have the same relative value.

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[1] For method of calculating the coefficients see this *Journal*, Vol. VII. Pt. 4, p. 469.

Fig. 1

1	2
b	c
c	d
d	e
e	f

Fig. 2

1	2
$b+p$	$c+p$
c	d
d	e
e	f

Fig. 3

1	4
$a+p$	d
b	e
d	g
e	h

Suppose now a specific factor to occur not in performances 1, 2 and 3 but in 4, 5 and 6. Let the additional correlation produced by it be denoted by q. The correlations between columns 5 and 6 (Fig. 4) will be little affected, as the corresponding terms in both columns are influenced by q to the same degree. But in the case of the correlation between columns 1 and 4 (Fig. 5), there will be a decrease, because q will tend to turn $h + q$ and $g + q$ in column 4 from small to big values, so that they will no longer correspond with c and d in column 1 (these being the smallest values). If the mean variation in column 4 is small while q is large in comparison with d and e, the correlations between columns 1 and 4 may even become negative.

Fig. 4

5	6
d	e
e	f
f	g
$g+q$	$h+q$

Fig. 5

1	4
a	d
b	e
d	$g+q$
e	$h+q$

Fig. 6

1	4
$a+p$	d
$b+p$	e
d	$g+q$
e	$h+q$

Suppose now both the specific correlations p and q to be present,

p affecting the inter-correlations between 1, 2 and 3, and q those between 4, 5 and 6. If p and q are both small, the correlation between 1 and 4 (Fig. 6) will be decreased, because opposite ends of the columns are being increased, but it may still be positive. If, however, both are large, it must become negative and eventually approximate to -1. Thus p alone has little influence upon any of the correlations between the columns, but p in conjunction with q increases the negative correlation between two columns.

TABLE IV.

	1	2	3	4	5	6	7	8	9	10
1		·95	·90	·85	·80	·75	·70	65	60	·55
2	·95		·85	·80	·75	·70	·65	·60	·55	·50
3	·90	·85		·75	·70	·65	·60	·55	·50	·45
4	·85	·80	·75		·65	·60	·55	50	·45	·40
5	·80	·75	·70	·65		·55	·50	·45	·40	·35
6	·75	·70	·65	·60	·55		·45	·40	·35	·30
7	·70	·65	·60	·55	·50	·45		·35	·30	·25
8	·65	·60	·55	·50	·45	·40	·35		·25	·20
9	·60	·55	·50	·45	·40	·35	·30	·25		·15
10	·55	·50	·45	·40	·35	·30	·25	·20	·15	

Let us take a numerical example in order to make clearer the above considerations. This is given in Table IV, which consists of the hypothetical correlations between ten performances produced solely by a general factor. In the above table it is assumed that in a hierarchy the coefficients decrease arithmetically. The correlations both in rows and columns decrease regularly by ·05. Mr Burt has pointed out to me that a true hierarchy could be constructed by multiplying each of the saturation coefficients successively by the rest. The above table, however, is intended for purposes of illustration only. As in every column the correlations decrease regularly from top to bottom, all the correlations between the columns will be $+1$. Now suppose a factor causing an increase of p in the inter-correlations to occur in performances 1, 2, 3, ... 7. The inter-correlations between columns 1, 2, ... 7 will not be affected at all, while those of columns 1, 2, ... 7 with 8, 9 and 10 will only be slightly reduced, *e.g.*, ·99, and this follows no matter what the value of p.

But suppose a factor causing an increase of q in the inter-correlations to occur in performances 8, 9 and 10; this will invariably cause a reduction in the values of the correlations between columns 1, 2, ... 7 with 8, 9 and 10, and the larger q is, the greater will be the reduction.

Thus when $q = \cdot 25$ $r = \cdot 57$
,, $q = \cdot 50$ $r = -\cdot 35$
,, $q = \cdot 75$ $r = -\cdot 59$

Suppose p and q to be present simultaneously, the correlations between columns 1, 2, ... 7 with 8, 9 and 10 will be still further reduced[1].

Thus when $p = \cdot 25$ and $q = \cdot 25$ $r = \cdot 24$
,, $p = \cdot 25$ and $q = \cdot 50$ $r = -\cdot 62$
,, $p = \cdot 25$ and $q = \cdot 75$ $r = -\cdot 81$

If Table I, from which Table II is derived, is examined, it will be found that the subjects can be divided into three groups, whose inter-correlations are distinctly higher than the correlations between the subjects of one group and those of another. The first group comprises geography, science, history and arithmetic; the second, composition, reading and spelling; the third, writing, painting and needlework. If each group of inter-correlations is enclosed in a square and the sides of the squares are continued across the table, a number of groups of correlations are obtained. The groups of inter-correlations are, for clearness, surrounded by double lines, and are lettered *A*, *C* and *F*. The remaining groups lettered *B*, *D* and *E* consist of the correlations of the subjects of one group with those of another.

Table II can also be divided into groups to correspond with those of Table I. For the purpose of comparison, the average correlation for each group in Table II was found and the results are given in Table V. At a glance it will be seen that the average of group *A* is higher than that of group *B* or *D*; that of group *C*, than *B* or *E*; and

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[1] I have assumed that the increment produced by the same specific factor is always the same in amount. Mr Burt, on reading my manuscript, pointed out to me that this might be taken as implying that the effect of a specific factor was simply to add the specific correlation to the pure correlation due to the 'hypothetical general factor.' He has suggested that, to reconstruct the table of composite ('total') correlations, the following equation might be used:

$$r_{12} = r_{1_g} \cdot r_{2_g} + r_{12 \cdot g} \sqrt{(1 - r^2_{1_g})(1 - r^2_{2_g})}.$$

Strictly, therefore, the increment due to p would be

$$r_{12\ g} \sqrt{(1 - r^2_{1_g})(1 - r^2_{2_g})}.$$

that of group *F*, than *D* or *E*. These higher averages suggest that there is a specific factor common to all the performances in each group.

TABLE V.

<table>
<tr><th></th><th>1</th><th>2</th><th>3</th><th>4</th><th>5</th><th>6</th><th>7</th><th>8</th><th>9</th><th>10</th></tr>
<tr><td>1
2
3
4</td><td colspan="4">C. = ·92
Uno. = ·81
A</td><td colspan="3">C. = ·64
Uno. = ·61
B</td><td colspan="3">C. = – ·56
Uno. = – ·21
D</td></tr>
<tr><td>5
6
7</td><td colspan="4">B</td><td colspan="3">C. = 1·00
Uno. = ·83
C</td><td colspan="3">C. = – ·89
Uno. = – ·46
E</td></tr>
<tr><td>8
9
10</td><td colspan="4">D</td><td colspan="3">E</td><td colspan="3">C. = ·93
Uno. = ·44
F</td></tr>
</table>

The figures given are the corrected and uncorrected averages of the groups in which they stand.

We have already considered the effect of specific correlations *p* and *q* occurring at the beginning and end of a table; now let us consider the influence of one occurring in the middle.

Suppose that performances 5, 6 and 7, Table IV, involve a further factor, causing a specific correlation *s*, and thus increasing their inter-correlations. The general effect of *s* will be to reduce all positive correlations between all the columns except the inter-correlations between columns 5, 6 and 7, and to increase all negative ones. This will cause a difference in the values of the correlations between groups *D* and *E*, Table V. Group *D* is not affected by *s*, but group *E* is, and its negative correlations will be increased.

If Table I is a simple illustration of the unifocal theory, then Table II obtained by correlating the columns of Table I should resemble Table VI *A*. If it illustrates the multifocal, it should resemble Table VI *B*, where *h* stands for a high correlation and *l* for a low one. But if it is an illustration of the combination of the conditions assumed in these two theories, and if there are three specific factors causing specific correlations *p*, *q* and *s*, the table obtained by correlating the columns will resemble Table VI *C*, where *h* stands for a high correlation and *m* for a medium one. Thus if the last theory be the true one, the correlations in groups *A*, *C* and *F*, Table V, will approximate to + 1; those in group *B* will have a positive value, decreasing with increase in the value of *s*. Those of *D* and *E* will be negative, those of *E* always being larger than those of *D*.

TABLE VI.

A

	+1	+1	+1	+1	+1	+1	+1	+1
+1		+1	+1	+1	+1	+1	+1	+1
+1	+1		+1	+1	+1	+1	+1	+1
+1	+1	+1		+1	+1	+1	+1	+1
+1	+1	+1	+1		+1	+1	+1	+1
+1	+1	+1	+1	+1		+1	+1	+1
+1	+1	+1	+1	+1	+1		+1	+1
+1	+1	+1	+1	+1	+1	+1		+1
+1	+1	+1	+1	+1	+1	+1	+1	

B

	+*h*	+*h*	−*l*	−*l*	−*l*	−*l*	−*l*	−*l*
+*h*		+*h*	−*l*	−*l*	−*l*	−*l*	−*l*	−*l*
+*h*	+*h*		−*l*	−*l*	−*l*	−*l*	−*l*	−*l*
−*l*	−*l*	−*l*		+*h*	+*h*	−*l*	−*l*	−*l*
−*l*	−*l*	−*l*	+*h*		+*h*	−*l*	−*l*	−*l*
−*l*	−*l*	−*l*	+*h*	+*h*		−*l*	−*l*	−*l*
−*l*	−*l*	−*l*	−*l*	−*l*	−*l*		+*h*	+*h*
−*l*	−*l*	−*l*	−*l*	−*l*	−*l*	+*h*		+*h*
−*l*	−*l*	−*l*	−*l*	−*l*	−*l*	+*h*	+*h*	

C

	+*l*	+*l*	+*m*	+*m*	+*m*	−*m*	−*m*	−*m*
+*l*		+*l*	+*m*	+*m*	+*m*	−*m*	−*m*	−*m*
+*l*	+*l*		+*m*	+*m*	+*m*	−*m*	−*m*	−*m*
+*m*	+*m*	+*m*		+*l*	+*l*	−*h*	−*h*	−*h*
+*m*	+*m*	+*m*	+*l*		+*l*	−*h*	−*h*	−*h*
+*m*	+*m*	+*m*	+*l*	+*l*		−*h*	−*h*	−*h*
−*m*	−*m*	−*m*	−*h*	−*h*	−*h*		+*l*	+*l*
−*m*	−*m*	−*m*	−*h*	−*h*	−*h*	+*l*		+*l*
−*m*	−*m*	−*m*	−*h*	−*h*	−*h*	+*l*	+*l*	

Now let us turn to the actual values of the averages of the six groups in Table II. These are given in Table V. The first figure in each group is the corrected average of the correlations in that group; the figure below is the average of the uncorrected ones. It will be seen that this table satisfies all the conditions necessary to justify its being regarded as an illustration of the combination of theories 2 and 3.

Let us take *p*, *s* and *q* at various particular values, and calculate the ensuing values for the six groups. The results are given in Table VII. The columns headed *p*, *s* and *q* give the assumed values of the superposed specific correlations, while those headed *B*, *D* and *E* give the corresponding values of the correlations in groups *B*, *D* and *E*.

(Groups A, C and F are not given as they will always be $+1$.) For example, the fifteenth line of the table means that, if $p = \cdot 25$, $s = \cdot 50$ and $q = \cdot 75$, the correlations in group B will equal ·31, in D $-\cdot 41$ and in E $-\cdot 83$. The only values in Table VII which approximate to the experimental ones given in Table V are those of lines 3, 12 and 21. Of these, line 3 is the nearest. Group B is somewhat high and demands that s and perhaps p should be slightly increased. If we increase s a little, we get, when

TABLE VII.

	p	s	q	B	D	E
1	0	·25	·25	·74	·57	+·15
2	"	"	·50	"	−·39	−·67
3	"	"	·75	"	−·59	−·86*
4	"	·50	·25	·48	·57	−·27
5	"	"	·50	"	−·39	−·83
6	"	"	·75	"	−·59	−·83
7	"	·75	·25	·39	·57	−·38
8	"	"	·50	"	−·39	−·85
9	"	"	·75	"	−·59	−·82
10	·25	·25	·25	·63	·70	+·15
11	"	"	·50	"	−·15	−·67
12	"	"	·75	"	−·41	−·86*
13	"	·50	·25	·31	·70	−·27
14	"	"	·50	"	−·15	−·83
15	"	"	·75	"	−·41	−·83
16	"	·75	·25	·14	·70	−·38
17	"	"	·50	"	−·15	−·85
18	"	"	·75	"	−·41	−·82
19	·50	·25	·25	·57	·73	+·15
20	"	"	·50	"	−·08	−·67
21	"	"	·75	"	−·33	−·86*
22	"	·50	·25	·26	·73	−·27
23	"	"	·50	"	−·08	−·83
24	"	"	·75	"	−·33	−·83
25	"	·75	·25	·08	·73	−·38
26	"	"	·50	"	−·08	−·85
27	"	"	·75	"	−·33	−·82
28	0	·30	·75	·69	−·59	−·89

$$p = 0, \quad s = \cdot 30 \text{ and } q = \cdot 75;$$

$$\text{then } B = \cdot 69, \quad D = -\cdot 59 \text{ and } E = -\cdot 89.$$

All these values agree within the limits of the probable error.

In order, then, to explain the peculiarities of Tables I and II, it is necessary to assume the presence of one factor common to all the performances; of one large specific factor, common to performances 8, 9 and 10; of a second smaller specific factor common to performances 5, 6 and 7; and possibly of a third very small factor common to performances 1, 2, 3 and 4[1]. Each of these four factors will now be dealt with in turn.

The existence of a general factor is in agreement with previous evidence. For example, Professor Spearman published a table of correlations between school subjects showing the hierarchical arrangement some years ago[2]. With actual tests of school abilities, Mr Burt has found a 'general educational ability,' apparently combined with specific educational abilities (arithmetical, linguistic, literary, and manual) similar to the combination of the 'hypothetical general factor' with subordinate group-factors in his intelligence tests[3]. Dr Starch[4] has published recently another table of school subjects also showing a hierarchy. Nevertheless, in many cases, the general factor has escaped notice, and investigators have regarded each of the subjects of the school curriculum as highly special, on account of the low correlations which have been found between them. These, however, have probably been due to the lack of reliability in the material used. School marks are very frequently inconsistent. Professor Spearman, also, in later tables, was unable to obtain the hierarchy, but this, he thought, was due to the fact that, in the schools from which he obtained his material, after a certain age the children specialise more or less in classics, science or mathematics. The present case is free from this complication, as the instruction in the elementary schools from which these marks were obtained is quite general.

Thus although every correlation in Table I has a significantly positive value, and this together with the correlations of Table II show clearly the existence of the general factor, yet there are discrepancies to which neither of the above explanations apply. The largest of these discrepancies is that which in the theoretical discussion was explained by the factor q. It has been found usually, that a comparatively small difference between psychological tests is sufficient to eliminate practically all correlation between them due to their

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[1] It is possible to determine the values of p, s and q by means of the formula for the correlation of sums (this *Journal*, Vol. v. Pt. 4), and the following mathematical relations:

$$\cdot 64 = f(ps),$$
$$-\cdot 56 = f(pq),$$
$$-\cdot 89 = f(sq),$$

and the results should approximate to the empirical values given above.

[2] C Spearman, "General Intelligence Objectively Defined and Measured," *American Journal of Psychology*, 1904

[3] C. Burt and M. Bickersteth, *Some Results of Mental and Scholastic Tests.* Cf. Burt, this *Journal*, Vol. III. 164.

[4] Daniel Starch, *Journal of Educational Psychology*.

specific resemblance, leaving only that due to the general common element g; in other words, the range of specific abilities is very small. Up to the present only one or two abilities have been noted of which this is not true to the same extent. Both Mr Burt and Dr Abelson found memory and motor ability to be of this character, but not to a very marked degree. But Table II shows that subjects 8, 9 and 10 are all influenced by one large factor which we have called q. As the abilities exhibiting this large additional and specific factor are technical subjects, which involve skilled motor operations, q can scarcely be other than this motor element. It is an example of a specific factor of a distinctly less narrow range than any observed previously. The definite proof of the existence of the motor factor is of importance from the pedagogic point of view. We can now be sure that the statement that certain children are clever with their fingers may have a definite meaning; and it may be desirable for teachers to discriminate these children from others, and to regulate their education and careers accordingly; for it not infrequently happens that these skilful children are lacking in general ability. Bagley, in the *American Journal of Psychology*, 1901, published a paper on "Mental and Motor Ability in School Children." The motor index was obtained from the average of tests in the dynamometer, trillings of a key, tracing of a line, target test, and automatograph. The mental index was obtained from school marks and teachers' estimates. One of his conclusions is as follows: "Under the conditions of the investigation, and with the children that were tested, a general inverse relation was found between mental and motor ability." This, however, is going too far, and later, Thorndike pointed out an error in the calculation and showed that the abilities were really independent.

The specific factor s in the three subjects, composition, reading and spelling is quite small, yet it is one to be expected *a priori*. This expectation was, in fact, the reason why these three subjects were placed together in the table, and this particular correlational possibility tried. All three subjects are concerned with written words and their significance or meaning, and this is probably the nature of the factor s.

If the factor p exists at all, it obviously has a very small value; and this again is what would be expected *a priori*. One cannot think of any specific factor which is likely to be common to geography, science, history and arithmetic and yet not to the other school subjects. In some schools, instruction in history and geography is correlated, and this, of course, might be expected to cause a higher correlation coefficient between these two subjects, but this was not the case in the schools from which these marks were obtained. If p is given the value 'zero,' the empirical values for groups B and D are slightly too high, and a small value lying between ·05 and ·10 must be given to p in order to make them coincide exactly with the experimental values.

So far, we have assumed that the general factor common to all the school subjects is simply identical with the ordinary g, or the factor common to all abilities. But it is conceivable that it may be more complex. The children's success at school may partly depend on their general ability, but in some degree also on some special capacity for, or attitude towards, school work. In order to determine the value of any such correlation, an endeavour was made to measure the correlation between the subjects due to g only. Two tests, not expected to have any specific correlation, were given. One required the re-arranging of disordered words into sentences, the other the writing down of the opposites of a series of words read aloud. Any correlation between these two tests was expected to be due to g only. By correlating them with one another, and then with each of the school subjects, it was possible to determine the correlation of each school subject with g by means of the formula

$$r^2_{xg} = \frac{r_{xa} r_{xb}}{r_{ab}},$$

where r stands for the correlation between any two performances indicated by the small letters, x for any school subjects and a and b for the two tests measuring g. Finally, the theoretical correlation due to g alone can be obtained by multiplying together the correlations with g found for each. If the experimental values are higher than the theoretical, this is an indication of a factor other than g.

The experimental values were found to be slightly higher. The average experimental value for group A is ·74, while the theoretical is ·68. However, not much confidence can be placed in this result, as it was found that there is a specific correlation between the two tests used to measure g. This had to be eliminated, making the determination much more indirect. But it is quite certain that if some other common factor, such as 'school zeal,' exists in addition to g, its influence, in the present case, is so small as to make it practically negligible.

Finally, to summarise, the results clearly indicate the existence of

(1) The general factor.

(2) One large additional complication, the motor factor.

(3) One small additional complication, the association between written words and their meanings.

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THE PROCESS OF NEGATION.

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I.

The purpose of the experiments described in this paper was to discover if there was any psychological distinction between the affirmative and negative judgment. The further problem of the relations existing between the two forms was also kept in view. These problems arose out of an old interest in Formal Logic, coupled with a growing realization of the inadequacy of that logic as a description of human thought.

Seven observers were available. Of these the five represented in the tables by the letters *A* to *E* were all trained in varying degrees.

It will be seen from the protocols quoted that the group of subjects displayed considerable variation in their characteristics. Observers *M* and *N* were untrained, and in consequence I have kept their results separate from the rest, and have been reluctant to use their reports in support of any contention. *M* is extremely good in certain parts of experimental psychology, but tests of the present kind were entirely new to him, and he failed to adapt himself. *N* has a natural talent for introspection and in many ways must be counted the equal of the trained subjects. But, not being acquainted with the psychological character of the problems under investigation, her reports always aim at completeness, with the result that the relevant points are often forgotten and nearly always buried in a mass of unimportant detail.

Five varieties of test were employed in the following order:

(1) To apply an epithet (suitable or unsuitable according to instruction) to a noun. (Three sittings.)

(2) To judge the correctness of a numerical statement. (Three sittings.)

(3) To judge the correctness of a verbal statement. (Three sittings.)

(4) To form a sensible and true proposition by supplying a predicate. (Four sittings.)

(5) To judge the correctness of a picture. (One sitting.)

D missed the last two sittings, the remainder of the observers completing the fourteen. Each sitting consisted of ten experiments. The pictures were displayed by a tachistoscope. The other stimuli were typed on cards and displayed in an exposure frame. The reaction times were taken either with a stop-watch or automatically by means of a lip-key in series with the shutter of the frame. But as no very reliable apparatus was available the times were only taken for control purposes, and have not been tabulated.

It will be observed that the tests fall into two groups. In one the observer has to supply a predicate, in the other he has to judge the correctness of a presented proposition or picture. Ignoring the affirmative cases, the former group may be conveniently termed "Negatives of Construction" (Series 1 and 4), and the latter "Negatives of Denial" (Series 2, 3 and 5). The results will now be discussed under these two heads.

II.

Negatives of Construction: (a) Series 1. *Suitable and unsuitable epithets.*

The nature of the experiments is described in the instructions read to each observer:

> "The test will be one of controlled association....The general instruction, or task, holding throughout the sitting, is to react with an epithet. The special instruction is to respond with a suitable or unsuitable epithet. This will be prescribed before each exposure."

This series of tests was intended primarily to afford the observers practice in the type of introspection required. The form was selected as involving the essentials of predication. The task differs from that of the companion series (4) in the absence of determination as to propositional form, and in the fact that the predicative aspect is not stressed. The mental process involves judgment, but the judgment may be very inchoate. The stimulus words employed were selected under certain heads or topics,—trivial, geographical, historical, abstract or ethical. Each set contained ten words, five being used for 'unsuitable' and five for 'suitable' epithets. The order was arranged to give an opportunity for a fairly complete establishment of the task in the middle of the sitting, while it constantly varied at other points. None of the observers noticed the arrangement. The following words form the second set, those in italics demanding unsuitable epithets:—*Armada*, Peninsula, *Ethics*, *Mouse*, Parliament Act, Ceylon, Faith, *Influenza*, Walpole, *Motor*. An inspection of the results shows for the trained group 135 correct answers, 7 incorrect, and 13 complete failures. The corresponding figures for the untrained observers are 37, 7, 16. A certain amount of licence is necessary in deciding which responses are correct, for in some cases an apparently incorrect epithet proves rational when taken in conjunction with the processes leading up to it. Taking now the correct replies and classifying them as *sensible* or *trivial*, the following distribution is found:—from the trained group there are 68 sensible and 1 trivial replies amongst the 'suitable' epithets, and 47 sensible and 19 trivial amongst the 'unsuitable.' The corresponding figures for the untrained group are 19 and 1, 12 and 5. The correct responses are now classified with respect to the manner in which they were obtained. It should be noted that the term 'immediate' means without the intervention of another epithet. Thus the case in which the epithet is simply the verbal translation of an image which occurs directly upon exposure would be classified as immediate.

TABLE I.

	Suitable				Unsuitable				
Observers	Immediate	Mediated by suitable	Mediated by unsuitable	Unclassified	Immediate	Mediated by suitable	Mediated by unsuitable	Unclassified	Total
A	13	0	1	0	4	6	0	3	27
B	11	4	0	0	0	12	0	3	30
C	10	0	0	1	3	2	0	5	21
D	12	2	0	1	2	10	0	3	30
E	10	4	0	0	3	7	0	3	27
Totals	56	10	1	2	12	37	0	17	135
M	7	0	0	0	1	5	0	2	15
N	9	3	1	0	2	4	0	3	22
Totals	16	3	1	0	3	9	0	5	37

The outstanding point of this table is the variation in the proportion of immediate responses in the two main classes. Considering only the trained group, it will be seen that while 81 per cent. of the classified responses are immediate when a suitable epithet is demanded, the percentage falls to 25 in the case of unsuitable epithets. Further, in the latter case the immediate responses are of a very trivial character, and in consequence cannot properly be compared with the other cases which are genuine examples of the thought process. Even if the figures of this column were much larger, the character of the processes involved would render it impossible to establish from them any conclusion as to the nature of the normal negative judgment. So far, then, the figures seem to bear out what is a commonplace of logical theory, that in the end a negative judgment is always based upon an affirmative one. But the real significance of the figures can only be judged in the light of the introspections, and here we enter a sphere in which the schemata *S is P* and *S is not P* have no place.

The following examples in which unsuitable epithets have to be supplied are typical of many others:

A. Mediterranean—*red.* "Visual image of intensely blue sea. Then in inner speech, 'Red is not blue.'"

B. Cromwell—*weak.* "Immediately there arose a not very distinct visual image of a man in armour on horseback, which suggested 'strength.' Word strength brought up memories and knowledge that he was strong, and I changed over to the opposite (I do not know how). Conscious afterwards that it was unsuitable."

D. Cromwell—*weak.* "Image of helmet and breastplate with figure in them. The word 'strength' occurred and then the reaction....Was pleased at its suitable unsuitability."

D. Influenza—*healthy.* "I thought, I cannot exactly say how, that 'healthy' was unsuitable. I can't find imagery or anything that can be described. The word simply seemed to fit in with the instruction."

E. Mediterranean—*pink.* "Visual image of deep blueness. Searched for another colour. I hate pink: feeling of pleasure when word came."

The above protocols are typical of the majority. There is no need to quote further, since the process is on the whole what one would anticipate. The process is one of twofold association, first the direct association of subject and attribute, and then the association with a word contrasting or co-ordinate with it. If we may regard the process as being at bottom one of judgment, we may symbolise it thus:— $S \rightarrow P_1 \rightarrow$ *non-P*, or $S \rightarrow P_1 \rightarrow P_2$. That is, the unsuitable epithet may be contradictory to the suitable one first thought of either generically or specifically. Thus P_2 is a species of the genus *non-P* within the universe P_1. It may be observed that the subjects very seldom change the universe in the midst of the process. It is a sign of defeat when such a merely formal negative as "Ethics—yellow" is offered. It is instances of this kind that were classified as 'trivial' at the beginning of this section, and the writer was pleasantly surprised to find how few occurred. It is evidently genuine thought in the majority of the cases. But the question arises, why are these processes negative? It is quite clear that there is nothing essentially negative about the process itself. The association of opposites is not radically different from other kinds and is best regarded as a form of association by contiguity (usually verbal). Again, it may be replaced by a co-ordinate association, as may be seen in some of the instances quoted. There is no theoretical reason why such a process should not lead to a 'suitable' epithet, and in point of fact it sometimes does.

B. Organ—*noisy.* "Seemed to be conscious of strains of church organ without hearing them. Thought of pleasant music, then more voluminous and thunderous. Memories of sitting in Psychological Lecture Room and hearing barrel organ: feeling of amused disgust and cynicism. Word 'noisy' came quite mechanically."

Another important point to notice is that in no protocol is there mentioned any aspect of negation in the process. This can scarcely be an oversight. The observers were explicitly asked to note it, and some of them seemed puzzled at their failure. They speak of "changing over to the opposite" and confess that they cannot say how it occurs. *B* says that in the unsuitable cases the end of the process is usually vague: the correct word simply comes into consciousness. Yet we shall see later that they were competent to indicate, if not to describe, phenomena constituting a consciousness of negation. It seems necessary to believe that there was nothing of the kind in this set of experiments. So the negative judgment seems to have vanished.

The clue is afforded by *D* (an unusually careful and detailed observer) in two cases quoted above. "Was pleased at its suitable unsuitability." "The word simply seemed to fit in with the instruction." On analysis it becomes clear that all the epithets given are really suitable, since the true starting-point of the thought process is the task prescribed.

We find ourselves, then, brought back to Watt's definition of the judgment. The starting-point of the process is the task which determines it, and since the reaction is in accordance with the instruction, we have no psychological criterion for distinguishing affirmation and negation. The negation is found only in the experimenter's instruction. The process may be represented symbolically. The initial point of the process is the task, which at first is indeterminate: it is to apply a suitable or unsuitable epithet to whatever term is exposed. This may be represented by *A* or *N*. The exposure of the stimulus word narrows this down, and here we have the true subject of the judgment,

$A + S$ or $N + S$. Then appears in consciousness the first moment of response, which is connected with S rather than A or N. In this sphere it is 'suitable,' and in the one case the process may terminate here; $A + S \rightarrow R_1$. Or R_1 may give rise to R_2. In the cases where the instruction demands an unsuitable epithet the task controls the next step, so that the whole process becomes $N + S \rightarrow R_1 \rightarrow non\text{-}R$, or $N + S \rightarrow R_1 \rightarrow R_2$, the link between R_1 and its successor being an association of a quite ordinary type. Sometimes the intervention of the task is quite explicit:

N. Mediterranean—*pink*. "Picture of a very large open space of sea, very sunny, hot and blue. Wanted to say 'blue,' and then task recurred and 'pink' came."

In this protocol the machinery of the response is made quite clear. Another point may be noticed. The essential link need not be intellectual, for feelings serve equally well:

B. Home Rule—*abominable*. "Vague image of Irish Sea and east of Ireland. Feelings, mostly a sort of disgust, summed up in word. Decision to react was devoid of imagery and carried by feeling."

C. Home Rule—*frightful muddle*. "Very little verbal imagery, almost entirely feeling, a jarring discomfort throughout body. Much the same feeling as of something of which one is utterly sick of hearing."

But whatever may be the nature of the intermediate term the response is 'suitable,' and this form of test gives us no instance of a negative judgment. That is, within the sphere covered by the experiments the distinction of affirmation and negation does not exist so far as the process itself is concerned.

III.

Negatives of Construction: (b) Series 4. Completion of Propositions.

The following instructions were read to each observer:

"You will be shown an incomplete proposition which you are required to complete. The subject and copula will be presented with signs of quality and quantity: you are to supply the predicate. You may modify the form of the copula, provided the quality and quantity remain the same."

The stimulus words were taken at random from a dictionary, and, with the addition of the other necessary words, typed on cards for exposure, thus, "All empires are...." The order in which the various logical forms occurred was determined by drawing cards which bore the usual symbols *A*, *I*, *E*, *O*. Each form appeared ten times. There were twenty stimulus words, each of which occurred twice, but never in the same sitting, and seldom in successive sittings. It was found possible to arrange that the word should appear in logically contradictory forms, except in two instances which appeared as contraries. As examples of the character of the stimuli we may take the first six of the second set.

"Some liquids are...."	"Some opinions are...."
"Some Moslems are...."	"No philanthropists are...."
"All nerves are...."	"Some reptiles are not...."

The general character of the experiment is similar to the one already described, but it stresses the fact that the term to be supplied has to be a predicate, and is more akin to an ordinary thought process. The task is more determinate, and the specific instruction is presented with the stimulus word. There is therefore no chance for the establishment of a 'set' before the exposure. The preliminary classification of the protocols of each type is given in the same form as that in the last section, but classification is more difficult. The observers were now accustomed to the experiments and were bent on giving very sound answers. The processes therefore become long and the introspections tend to lose clearness, the more so that there are many false starts.

TABLE II.

Type of proposition	*A*			*I*			*E*			*O*			
Observers	Immediate	Mediated by affirmative only	Mediated by negative	Immediate	Mediated by affirmative only	Mediated by negative	Immediate	Mediated by affirmative only	Mediated by negative	Immediate	Mediated by affirmative only	Mediated by negative	Unclassified
A	2	4	3	0	8	1	1	7	0	1	7	1	5
B	2	6	1	0	6	4	1	2	1	2	6	0	9
C	1	6	1	0	8	0	1	6	0	4	5	0	8
D	1	5	3	0	6	0	3	1	0	2	3	0	6*
E	0	5	4	4	5	0	2	3	0	3	4	0	10
Totals	6	26	12	4	33	5	8	19	1	12	25	1	38
M	0	2	1	3	3	0	1	0	2	2	4	0	22
N	2	4	0	3	5	1	0	6	1	1	5	0	12
Totals	2	6	1	6	8	1	1	6	3	3	9	0	34

* *D* missed one sitting.

The one difference we found between affirmative and negative judgments—the proportion of immediate answers—has disappeared.

The greater proportion of immediate answers is now to be found amongst the negatives, but the numbers are not sufficient to enable any conclusion to be drawn from this. The distribution is not enlightening, and results must again be sought in the protocols themselves. A set of protocols is therefore quoted which will sufficiently illustrate the processes involved in the affirmative judgments.

N. All reptiles are *crawly.* "Saw words with meaning, and felt something crawling round me. There was no other kind of imagery; I seemed to shiver within myself."

A. All castles are *fortified.* "Succession of images of castles; in each case the battlements attracted my attention, and suggested the reply."

E. All castles are *strong.* "The word 'impregnable' came very soon. Then, in internal speech, the words 'Not all.' Very clear image of castle on a hill. Wanted to say 'Built on hills.' Against this came faint image of castle on plain with water round it. Came back to former image and noticed stones of which it was built. Then 'strong' came."

A. All nerves are *fibrous.* "Immediate inclination to say 'sheathed,' with image of a diagram. Then came the image of an unsheathed nerve, and an inhibition of the vocal organs. Tried pairs of words that would not do. Examined image more carefully and observed it was composed of fibres."

N. Some opinions are *worthless.* "Saw words without taking them in with 'worthless' at end (*i.e.* as image), and as I read it said it, with passing mood of utter contempt." (1·6″.)

C. Some liquids are *mobile.* "Was brought up by image of a liquid resembling alcohol,—very brilliant, and it seemed to flow freely when shaken, though I don't remember any shaking. This was contrasted with an image of treacle, a dark mass slowly trickling down."

A. Some Moslems are *Arabs.* "Image of interior of a mosque, empty to begin with, then suddenly filling with men in Arab dress."

B. All diamonds are *made of carbon.* "Word 'diamond' accompanied by faint image of a diamond shape. Almost instantaneously came the *knowledge* (I cannot describe it in any other way) that they were made of carbon."

The various forms taken by the affirmative process may be gathered from these examples. In most cases there is the immediate rise of a content connected with the subject: if it is suitable the process is finished, except possibly for the translation into words. But there may occur an inhibition, presumably due to the influence of the quantitative aspect of the task. The first tendency is to respond *All S is P*, but this is checked either by the definite negative judgment "They are not," or by memory of a negative example, and the process starts afresh. This interruption of the process accounts for the number of cases in which the affirmative conclusion is mediated by a negative process. The differences between the *A* and *I* propositions do not concern us.

It should be noted that there is no reference in any case to a distinctive consciousness of affirmation.

The negative judgments must be examined more thoroughly. The comparatively large number of immediate responses to general propositions is misleading. One will serve sufficiently as a type.

E. No Moslems are *Christians*. "Word came automatically."

Evidently the response is simply the reproduction of an association already formed. There is no real construction here, and consequently such examples are useless for our purpose. We shall find the more genuine type of the negative judgment in those which are mediated by an affirmative process. By 'affirmative process' is meant the thought of some factor in the nature of the subject, whatever the thought process may be phenomenally.

A. No Moslems are *intemperate*. "Task recollected, and response inhibited for a time. Vague image of interior of a mosque. Thought 'forbidden to drink alcohol,' and cast round for a negative."

A. No wolves are *herbivorous*. "Clear image of a wolf-pack coming over snow out of some timber: it changed, and they seemed to be devouring some carcase. There was an inset of a wolf eating grass, which seemed absurd and not true of wolves."

This observer's thought is always predominantly visual, though he is quick to seize the case where the image is not the full expression of the thought. The character of the process is clear. The presentation of the subject brings up in thought some part of its character, which in its turn calls to consciousness another form of words expressing the opposite, or a contrasting idea. But, apart from the actual form of this final association which is determined by the task—its intervention is explicit in some cases—there is nothing to differentiate the negative judgment from the corresponding affirmative. In the last instance quoted there is a reference to a consciousness of negation, but this is the only example in the whole of this set. In the remainder of the cases the successive moments of the process seem to occur in much the same way as if they were the steps of a multiplication. I quote two more cases from different observers, which show the process very clearly.

B. No wolves are *tame*. "Word 'wolves' brought up a visual image which was very schematic except as regards the wolf itself. Then in some way associated with it was 'fierceness.' Then read the three words of the stimulus and expressed the quality (*i.e.* negation) by giving opposite of this meaning."

C. No Moslems are *wine-bibbers*. "Tried to recall formal Moslem beliefs. Thought of wine, and then gave response."

In the case of *O* propositions the responses were more easy. It would seem that 'some' simply brings us back to the indefinite form of the earlier tests. The memory basis of the 'immediate' response is well shown in the following protocols.

C. Some knives are not *sharp.* (1·8″.) "At once there came an image of a table-knife, new and blunt, with edge towards me, the width being very much exaggerated." (Here "not sharp" is evidently only a verbal equivalent for blunt.)

A. Some judgments are not *true.* "An image of Joseph's *Logic* opened in middle, and in the middle of the page were the words 'Some judgments are not true of reality.'"

There are two types of mediation found in this series. The quantitative instruction 'some' may be taken as meaning 'some, but not all,' or, and more frequently, as releasing the observer from the stringency of the universal form. Taking the former type we have such instances as the following.

B. Some reptiles are not *poisonous.* "Word 'reptile' the most prominent Read it as meaning a more general class of creature than snake, but including that as its chief species. Visual image of any snake. Then 'poisonous' came, and seemed to be associated with word 'snake.' Read remainder of sentence and recognised that word was suitable, knowing that some reptiles are harmless."

C. Some diamonds are not *transparent.* "Diamond connected with the Cullinan Diamond. Then 'black diamond' came up verbally. Thought that some diamonds might be of other colours, and then the judgment seemed to come up in antithesis to the first."

D. Some castles are not *surrounded with a moat.* "Visual image of Tonbridge Castle with the moat quite clear. Then conscious that some were without. Verbal image of 'what?' Then verbally 'Penshurst Castle,' followed by repetition of first visual image with the moat filled in."

These reports closely resemble the typical logical examples. On seeing the stimulus the observer thinks of some particular example which is followed by the thought of another differing in some quality. The process then concludes with the negative statement, which is formed by discovering a word which is opposite in meaning to one of the contents thought of. Thus "Some reptiles are not poisonous" is, for the thinker, only an alternative and equivalent statement to "Some reptiles are harmless." The real judgment had concluded with the latter. So, too, the actual judgment is "Some diamonds are paste," but under the government of the instruction it has to be changed to "Some are not real." Psychologically, the process is not essentially different from those of the second or indefinite type.

A. Some castles are not *in a state of good preservation.* "Visualised ruins of a castle. Remainder of process was an effort to find an opposite term to 'ruin.'"

D. Some reptiles are not *amphibians.* "Image of crocodile with tendency to react as above. Inhibited while thinking of different kinds. Conscious of universal 'reptile' and species. This was not definitely verbal nor visual, but an attitude towards the sentence. Then conscious that I was right, and reacted."

E. Some reptiles are not *quadrupeds.* "Visual image of a long extended thing on ground; then image in opposition coming up above it, as much like a sheep as anything. Main thing that struck me was its four-leggedness. The word 'quadruped' seemed to fit in."

An examination of the whole range of data from which the above examples have been quoted shows that in essentials the processes involved are the same as in the epithet tests. The forms of mediation only differ in being more complex and in occupying longer time. This complexity is due to the instruction, which emphasises the necessity of forming an intelligible proposition, and introduces the consideration of quantity. It is the latter point that chiefly affects the process. But the relation between universal and particular judgments is irrelevant to the writer's purpose. Confining ourselves to the question of negation, it must be repeated that no essential difference can be established between the affirmative and the negative judgment, so far as the mode of mediation of the response is concerned. Its characteristic is that it concludes with an 'association by contrast,' either real or verbal. But it cannot be held that such an association is specifically negative. Nor do we find any reference to a specifically negative experience, except in one protocol. But we have seen that such a consciousness of negation may occur in the midst of an affirmative judgment. In short, the distinction of affirmation and negation has disappeared here as in the earlier experiments. If we now consider the conclusion indicated by those experiments, and ask if it applies here, we find that *D* again throws light on the matter. "Conscious that I was right, and reacted." So *E* says, "The word 'quadruped' seemed to fit in."

Our conclusions are therefore the same as in Section II. The actual task set the observers was to respond in accordance with conditions prescribed. A variation in the conditions led to a corresponding variation in the response, but not to any essential modification in the process. The real judgment formed is not properly represented by the conventional formula *S is not P*, but by *P meets the case.* The starting-point of the process is the stimulus which gives the subject and the conditions which the proposition must fulfil as to quantity and quality. In some cases the subject calls up, by ordinary forms of association, a predicate, which then, under the influence of the determinant tendencies, excites a contrasting content for which a name is found. The sequence may be represented by $N + S \rightarrow P \rightarrow non\text{-}P = X$. In other cases the first predicate excites another which applies to other particular cases of the subject, and this is taken as representing the class contrary to the former: $N + S \rightarrow P \rightarrow Q \rightarrow non\text{-}P = X$. The judgment corresponding to the verbal proposition *S is not P* is better represented by the obverse form *S is non-P*, where *non-P* has a fairly definite content. Thus we find that the proposition "Some reptiles are not poisonous" means for the thinker "Some reptiles may safely be ignored." These experiments have therefore failed to give us a genuine case of the negative judgment, *except by accident*. The wider conclusions must be left until the other tests have been described.

IV.

Negatives of Denial: (*a*) *Series* 2. *To judge the correctness of a numerical statement.*

These tests, and the similar series, had two aims. On the one hand they were an attempt to track down negation as a conscious experience, on the other they attempted to answer the question whether it was necessary to form a contradictory judgment before denying the truth of the one presented. Arithmetical material was taken in the first instance as affording the clearest distinction possible of right and wrong. Of the thirty cards used half gave incorrect statements, and the order of presentation was varied to avoid any likelihood of expectation. With one exception the 'wrong' statements were constructed so that they could be judged wrong without actual calculation. Needless to say, the observers were not told this, and a careful study of the protocols in sequence shows that they did not realise the method adopted. The nature of the stimuli will be seen from the first five of the second set:

$249 - 138 = 111.$ $\quad 12 \times 5 = 60.$ $\quad 18 - 14 = 32.$
$74 \div 9 = 100.$ $\quad 17 + 4 = 21.$

The instructions read to each observer ran as follows:

> "You will be shown simple arithmetical statements. If they are correct react with 'Yes,' otherwise 'No.' The introspection should deal with the whole process, but especially with the actual moment of decision."

The 'right' examples had to be judged either from memory, or, more frequently, by complete calculation. The results, therefore, have little value for our purpose, and are only briefly considered in the following discussion. Taking the cases where the statement is correctly judged to be wrong we have:

TABLE III.

Observers	Immediately and before completely reading	Immediately after reading	After judging character of 'answer'	After definite calculation
A	0	4	6	4
B	1	7	3	3
C	3	6	2	3
D	1	9	2	2
E	0	5	4	6
Totals ...	5	31	17	18
M	2	2	4	0
N	3	6	2	3
Totals ...	5	8	6	3

The tabulation at once answers one of the questions proposed. Since half the responses were correctly given without any representation at all of what the right-hand member of the equation should be, it is evident that it is not necessary for a judgment to be formed prior to denial of the one presented. Comparing these figures with the 25 per cent. and 30 per cent. of the earlier tables, it would appear that we have here a different type of process. To judge that *S* is not *P* it is usually necessary first to judge that *S* is *P*, or that *S* is *Q*. But in the case of arithmetical statements it seems that we may judge that the proposition *S is P* is false without forming the judgment *S is Q*, or even the judgment *S is not P*. The problem as to how this is possible seems worth consideration. In a few cases the denial may result before explicit comprehension of the statement.

B. $2448 \div 12 = 19$. "It seemed a big sum, and I turned to glance at the right-hand side. Without cognising numbers was struck by its smallness as compared with the other, and immediately said 'wrong.'"

C. $74 - 69 = 56$. "Some hesitation about what it all meant. Without troubling about figures gave the answer, apparently from the mere look of the thing."

In these instances the reaction occurs before the figures are read, and yet it is correct. This form is uncommon, but those who read more fully may none the less answer immediately, *i.e.* without any thought of what the statement should be.

A. $74 \div 9 = 100$. "Fixated the division sign which monopolised attention. Then saw 74 and 100 and answered 'No' without seeing the divisor."

A. 239 ÷ 7 = 456. "Division sign recognised first, then the answer. Then looked at dividend and saw it was less than the quotient, and a feeling of wrongness occurred."

B. 74 − 69 = 56. "Struck by closeness of the two numbers. Had to read again to see if possible. The size of the answer seemed grotesque, and I said 'Wrong.' It seemed a kind of ill-balanced affair."

C. 239 ÷ 7 = 456. "Looked at 239 which seemed to have no relation to 456, and reacted. There was no thought process: it was purely a matter of feeling, unpleasantly toned, supplanted by pleasure at being able to answer quickly. The 7 had little to do with it."

D. 46 − 7 = 52. "At first saw thing as a whole. Then an effort to work something out, but almost immediately the absurdity was seen and all work was definitely abandoned. Then came the decision which was not purely intellectual; there was an affective side to it."

N. 239 ÷ 7 = 456. "When figures were seen there was a thrill of satisfaction. Saw 2 and 4 and felt it was wrong, and said 'No.'"

Two points stand out clearly in these reports. As already mentioned, no intellectual process intervenes between the apprehension of the numbers and the response. On this the protocols are convincing, for they not only fail to report such, they frequently explicitly deny its occurrence. The second point is the number of references to feeling. This term always needs to be regarded somewhat sceptically, for observers find it a very present help in time of introspective trouble. We had better regard it for the present as denoting certain obscure processes which are probably complex. But it would be absurd to carry this caution to the point of ignoring the affective reference altogether. The process is such that the observer is driven to describe it in quasi-emotional, or aesthetic, terms, and it can mediate the denial of the statement presented. The actual frequency of such references therefore becomes interesting. Taking the trained observers only, there are 21 cases where reference is made to affection in 36 immediate responses. The distribution is:

A	*B*	*C*	*D*	*E*
$\frac{2}{4}$	$\frac{7}{8}$	$\frac{6}{8}$	$\frac{4}{10}$	$\frac{2}{6}$

Two more examples are quoted.

A. 48 ÷ 10 = 58. "Saw sign first as + and was about to say 'Yes.' Then there was, as it were, a reverse flow over the top of the head, and I recurred to the sign with a feeling of absurdity at sum as wrong."

D. 239 ÷ 7 = 456. "Saw clearly answer, ÷, and 7, not the other figures. Visual image of 1 (9 − 7 = 1), but before seeing this knew I had to react with 'wrong.' There was no clear grasp of units nor imagery, simply a feeling as if brushing the whole thing aside."

This last phrase is, perhaps, the most significant in all the protocols. It indicates a form which the process of negation may take, a form clearly derived from the gesture of impatience or annoyance, which itself symbolises the actual removal of an offending object. If one may hazard a guess, the negation is carried in this instance by a process compounded of feeling and kinaesthetic imagery.

The figures in the third column of the table show the cases in which the responses were mediated by some kind of cognitive process. The answer of the sum was roughly forecasted, either with regard to its size, or as to what one of the digits must be. In the former case the approximation is often very vague and schematic, and independent of any detailed calculation, so that the process tends towards the immediate type.

A. 648 ÷ 3 = 7. "Division sign appeared first and then saw answer. There arose a consciousness, not expressed in words, that the answer must be three figures. This was partly carried by a visual image of three grey forms on a white ground which might stand for any figures."

D. 74 − 69 = 56. "There was no definite consciousness of precise figures, but what I might call a sense that 56 must be more than the difference between the other numbers."

The second type approximates to the cases of the fourth column, since they depend on partial calculation. Unfortunately the introspections are not so good in this group as in those already discussed. The observers fall into the trap of describing the thought process as they would describe a piece of calculation to a beginner in arithmetic. This does not altogether destroy their utility for the present purpose, but it renders them less interesting.

E. 648 − 3 = 7. "Read over sum without looking at answer. In internal speech said, 'Three into six.' Then, before the 'two' was ready to come, had got to '7.' I had expected three places: there was present a visual image of 'H.T.U.,' but not focally."

In the fourth column we occasionally meet with references to the quasi-affective determinant of the response. The following will serve as examples.

A. 134 − 28 = 242. "Recognised the subtraction sign. Then a feeling that answer must be less than 134 (something sinking and contracting in throat), and therefore less than 242."

A. 46 + 5 = 39. "Sign perceived first, then that 46 was greater than 39. Then a half-conscious process, 'Much more is 46 + 5 greater than 39.' This was not in speech form, but there was a feeling at the back of the throat as if something were rising and expanding."

B. 134 – 28 = 242. "Sign caught my eye first. Worked sum approximately, fixing answer as a little over one hundred. Saw answer given as over two hundred, with a feeling of surprise. This usually seems to prompt the response."

In the first two reports what is styled feeling has indisputably a sensory basis, but the last is unequivocal. Reports of this kind are much rarer in the third column than in the 'immediate' cases. But the fact that the affective process is not mentioned in any protocol is no proof that it was not present. In the immediate responses the affective process was practically all there was to report, while here there are more substantive and obvious processes which tend to attract the observer's attention. So affection may have been present more frequently than the reports suggest. If, however, we must admit that there is insufficient evidence that the affective process is important in these cases, it must be stated that there is nothing else reported which can be taken as a consciousness of negation. We are therefore driven to interpret this form of denial as a reversion to the Negative of Construction. The presented proposition *S is P*, on analysis, gives rise to the judgment *S is not P*, which is translated in accordance with the instruction by 'No.' The same may be said of the instances grouped in column 4. By some process of calculation a judgment is formed as to what the right-hand side should be. Is there any specific consciousness of negation? In some cases, at least, there is.

A. 74 – 69 = 56. "Recognised difference as 5. Looked at answer, felt its absurdity and said 'No.' The feeling was one of inclination to laugh at it."

E. 46 + 5 = 39. "Made myself do sum. By the time my gaze had reached the answer had 51 which was in some way over 39. Heard words 'Thirty-nine,' in a very surprised tone, with feeling that it was below 40."

N. 46 + 5 = 39. "Read statement as 46 + 5 = 51 and did not notice answer. Was about to say 'Yes,' when the 9 upset me and I felt angry."

Turning now to the correct examples, the only question that concerns us is whether we can find any process parallel to the affective constituent of negation, for the method of arriving at the decision, involving, as it does in nearly every case, some form of calculation, is not relevant to our purpose. The question is not easy to answer. In many of the protocols there is an explicit denial of any affective process, nothing being reported except the details of the process constituting the calculation. On the other hand there is an equally confident assertion of the presence of feeling in 28 cases out of 98. But even in these there is difficulty, for while in some cases the feeling is quite clearly connected with the affirmation, in others it may be the result of the working. The feeling in question is usually described as pleasure or ease, and this may quite well arise from the successful working, and not from the affirmation itself. But that there is at times a definite consciousness of affirmation or truth, and that this is in part at least affective, is clear from the following examples, the number of which could, as in other cases, be extended.

A. 48 + 32 = 80. "Read left-hand side in internal speech. Compared it with answer. Then a feeling of agreement, not pleasant nor unpleasant, a detached kind of feeling."

B. 36 – 12 = 3. "All felt familiar: looked at right and feeling increased, and I recognised answer as right. There was no kinaesthesis and no working out, but there was much feeling which was not pleasure, and there was no tension. It was simply recognition."

D. 144 × 2 = 288. "....There was certainly an affective side in this instance, viz., pleasure at the symmetry of the numbers."

E. 17 × 4 = 68. "Worked it out in internal speech. There was a feeling of pleasure, of fitness."

The mental processes dealt with in this section are, taken as a whole, clearly different in character from those which occurred in the earlier series. Where the task is to test the truth of a completed statement it is not necessary that a constructive judgment should be formed before denying the truth of the one presented. In this form of negation the negation does not necessarily depend upon the consciousness of an affirmative judgment. Further, we have found evidence of a process which is the conscious negation that we failed to find by other methods. This process is usually described by the observers as feeling, but we have seen that it often contains elements of sensorial origin. Two further examples of this may be quoted which possess additional interest from the fact that they occurred in the midst of affirmative processes. They are therefore more spontaneous and trustworthy than if they had occurred after an incorrect stimulus:

A. 14 × 3 = 42. "Immediate inclination to say 'No.' Then a drawing back sensation at the back of the throat, which relaxed on seeing that 14 × 3 = 7 × 6 = 42."

A. 249 – 138 = 111. "Read the whole card and wanted to say 'No.' Then a contraction at back of throat, implying that it was wrong to say 'No.' Worked sum; feeling of relief."

Here we have a description of the negation of an incipient negative judgment, and the sensorial origin of the process is unmistakable. The protocols also serve as further instances of the immediate negation, for which there is no real cognitive basis apparent. But if the negative consciousness frequently contains sensorial elements we must admit that in a large number of instances it is described in terms of emotion. It would be safest to describe it as an emotional attitude, the precise constitution of which we cannot at present determine. Where the denial is not immediate there is less evidence of this specific consciousness of negation, and the process approximates to the Negative of Construction. There is less evidence of a corresponding consciousness of truth or agreement, but we find a fair number of references to it. They occur chiefly when the stimulus is simple and the process short (*i.e.* where the reaction time is less than two seconds). So far, then, as we can trace it, it tends to occur when the judgment of truth is immediate. From the descriptions it would seem that this type of consciousness is less pronounced than the negative. The consciousness of affirmation resembles recognition, while the consciousness of negation may be described as a mental jar.

V.

Negatives of Denial: (*b*) *Series* 3. *Verbal propositions.*

The subject was informed:

> "These tests are similar to the foregoing, but verbal statements will be presented. No catch is contained in any of the statements, which should be considered in their most obvious significance. But the judgment of truth or falsehood should be a considered one."

The experiments were a form of the *Ausfragemethode*, and the writer would endorse the criticisms which have been urged against it. So would the observers, if one may judge from the remarks they let fall at the end of the series. Even the most practised of them constantly fell into the 'psychologist's fallacy,' and as they were skilled enough to recognise the fact they became exasperated or disheartened. But this fault does not impair the value of the protocols so much as it would if we had been concerned with the complete phenomenology of the thought process. So long as the observer reports *that* he thought of certain attributes of the subject, it does not greatly concern us *how* he thought of them. A more urgent difficulty was that the material was not so amenable to control as the numbers had been. If the propositions were to be really significant their truth must be in many cases a matter of opinion. So while the experimenter considered that fifteen of the stimuli were true and fifteen false, the observers differed. The responses have been accepted in all cases save those where the observer desired to withdraw on second thoughts. The propositions were distributed over the four logical forms, and the usual precautions taken to prevent expectation. The following, from the second set, are types of the material employed:

> "Some fanatics effect reforms."
> "No metals are liquids."
> "No religion is independent of faith."
> "All plants bear flowers."
> "Some revolutionaries are timid."

It seems convenient to classify the responses in a form similar to that of Table I.

TABLE IV.

	Judged correct				Judged incorrect			
Observers	Immediately	After affirmative process	After negative process	Unclassified	Immediately	After affirmative process	After negative process	Unclassified
A	4	11	2	0	3	10	0	0
B	6	3	2	2	2	12	0	2
C	4	10	0	0	2	10	0	1
D	1	12	0	1	1	11	2	2
E	3	9	2	0	5	6	0	1
Totals ...	18	45	6	3	13	49	2	6
M	1	7	1	1	2	13	2	3
N	4	7	0	1	7	3	2	6
Totals ...	5	14	1	2	9	16	4	9

There is nothing to be learned from these figures as they stand. They are widely different from those of Table III in the frequency of immediate negatives, and there is no significant difference between the affirmatives and negatives of the same table. It is necessary, therefore, to take the protocols in some detail, with the view of discovering whether the specific consciousness of negation that appeared in the mathematical tests is present here. And if the immediate negatives are of any value we may take it as proved that the negative of denial does not necessarily involve the prior formation of an affirmative judgment. The only interest that can lie in the actual figures of this column is that 25 per cent. is too large a proportion to be due to accident or carelessness in introspection. Taking now the affirmatives, the 'immediates' are typified by the following examples:

A. All sciences are systematic. "Actual judgment was 'Yes, or should be.' There was no imagery, but a comfortable feeling of agreement, and an internal repetition of the stimulus in a tone of affirmation."

C. Some paint smells strongly. "No verbal or kinaesthetic imagery at all.

Visual image of a painted surface: can't be sure whether I had an olfactory image, but actual smells seemed rendered more acute. Response came almost like feeling."

E. All birds fly. (1·6″.) "Kind of 'Of course they do' attitude. No imagery."

In these instances the reaction followed without any analysis of the stimulus, and there was a definite consciousness of truth, which is described in various ways. In the majority of the experiments the meaning of the stimulus has to be developed before the response is given.

A. No religion is independent of faith. "Thought of Paulsen's definition of Religion as a combination of faith and humility. A feeling of provisional acceptance in kinaesthetic terms, a sense of holding out hand for something which might be hot."

C. Some fanatics effect reforms. "The word 'fanatics' seemed to connect itself with teetotallers. I seemed to go through a fair amount of reasoning, but little in the way of imagery or speech. 'Narrowness' occurred in inner speech, with the judgment that some reforms need fanatics."

D. Some inferences are not syllogistic. "'Immediate' occurred at once. Thought of the criticism of syllogistic inference contained in Bosanquet's 'Logic' (visual image of an open book). Word 'Kant' occurred. Conscious that his mistakes were due to his respect for the syllogism: aware that it is never used, and reacted with intense consciousness of being right, a feeling of exultation that I need not bother."

N. Some fanatics effect reforms. "Straightway had most hazy notion of religious fanatics, people of extreme views. Then, not in words but just a feeling, came that they were bound to point the way to reforms. Reply came with pleasure."

The following example is typical of the few in which the analysis involves a process of negation:

A. No famines occur in England. "'Famine' brought up association 'India,' with strain at back of neck, indicating 'not in England,' and an expansive feeling implying 'greater than England.' Then 'potato famine in Ireland' as a single idea, with an image of a very withered potato. Then in internal speech 'Ireland is not in England.' Then, as no more instances appeared, agreed."

Once more we find contradictory reports as to the consciousness of affirmation. Some do not mention it at all, while others definitely assert its presence. So, too, in some reports 'feeling' is mentioned, in others it is flatly denied. But there is sufficient evidence to show that there is a specific consciousness of affirmation which is most clearly marked when the reaction does not depend upon intellectual processes. When analysis of the presented statement has to take place the psychological process tends to fall into the constructive form, in which the distinction of affirmation and negation does not exist. When the affirmative consciousness is described at all it is styled 'feeling,' though this, as we have seen before, needs further explanation.

We may turn now to the cases where the presented statement is judged to be incorrect. The immediate judgments are typified by the following protocols:

A. *All the Puritans were uncultured.* "Feeling of drawing back, and then in internal speech, 'Don't know much about the Puritans, but this seems too sweeping.' No conscious ground for denial except this."

B. *Some rivers flow uphill.* "Meaning conveyed by a not very clear image of a hill and a river flowing half-way up. Seemed to feel the difficulty it was experiencing, and this carried the absurdity of the sentence."

C. *Some sheep have green fleeces.* "Some sort of image of the wool of a sheep. Fleece seemed to turn green, causing incipient amusement."

N. *Some friendships are not sincere.* "A feeling or sensation of warmth: 'friendship' and 'sincere' seemed to stand out in a row by themselves, with the other words behind. Wanted to say 'Yes,' but the warm glow seemed to be over me, so that I gave answer against my will."

The following are examples of the mediated judgments:

A. *Some Orangemen are not opposed to Home Rule.* "Image of a procession of Ulster Volunteers. Then verbally, 'It is the essential characteristic of Orangemen to oppose Home Rule.' Sense that the two terms did not coincide,—a kind of gap between them."

B. *All ships have sails.* "Hasty glimpse of a ship in full sail, which changed to steamer, which conveyed wrongness. No feeling."

C. *All birds fly.* "Very schematic image of a bird with outstretched wings, like a cross. Formed judgment in inner speech 'All birds have wings.' Then thought of ostrich."

E. *No vertebrates have wings.* "Very faint image of spinal column, with ribs coming out Then image of bird with outspread wings. Feeling of pleasure."

There are fairly frequent references to feeling, but not so many as in the case of the arithmetical statements. The following are interesting and suggestive examples:

A. *Some worms have legs.* "Read twice before getting meaning. Then image of worm wriggling along ground. Then a sort of negative feeling—a strain at back of the head, I think—implying that it was untrue."

C. *All ships have sails.* "The judgment seemed to take me by surprise. Image of square-rigged ship which seemed to stop me thinking. Then an image of a submarine on the surface."

D. *Some sheep have green fleeces.* "Began to think of species of wild sheep. Visual image of a goat-like sheep (ibex). Came to conclusion that none had. Image of picture of a goat which was green. Then a feeling or sense that it looked ridiculous and reacted."

It will be observed that in some cases the phrases used to indicate the consciousness that the stimulus is wrong would do equally well for the affirmative consciousness. "Intense satisfaction" in the reaction cannot be considered as peculiarly negative in character. We find here a defect of the method employed. The task we desire to set is to judge whether the proposition presented is true or false. The task which tends to take primary rank for the observer is to answer the implicit question, "Is it true?" correctly. Consequently any specific consciousness that the proposition is wrong tends to be masked by the consciousness that the reaction is right. That is, the judgment constantly tends to be affirmative, and, moreover, of the constructive type. The latter difficulty arises in two ways. On the one hand, the proposition "*S is P* is false" is constructive and affirmative. On the other, where analysis of the stimulus takes place, *S is P* tends to be replaced by *S is not P*, and the reaction 'No' is simply a verbal translation of this. We encountered a similar difficulty in the case of Negatives of Construction, but reasons will be suggested in the concluding section for believing that there this was not merely a result of the experimental procedure, but a consequence of the natural form of thought. Here, however, it is indisputably the result of the method, which fails to reproduce the normal mental process. Fortunately, a good many of the protocols escape this pitfall. We get the normal and genuinely negative process when the stimulus is so striking as to block the efficacy of the instruction and cause a spontaneous judgment. To illustrate what is meant by blocking the instruction I give an example which appears among the failures of Series 4:

N. No wolves are ***hungry.*** "After exposure of stimulus I could see lots of green all round me, and big mouths open, without any animals. Wanted to say 'Nice,' but said 'Hungry.'"

In this case the determinant tendency is destroyed by the startling character of a new process. So we find that when the statement is at all striking and simple we have a reaction which is unmistakably negative. Sufficient examples of this kind have been quoted. It would be impossible to construct all the stimuli on the lines of "Some sheep have green fleeces." An atmosphere of absurdity would pervade the whole enquiry, and the replies would become more and more mechanical. But we find further spontaneous denials embedded in more complex processes, and these are perhaps the most useful of all, since they occur practically independently of the experimental procedure.

A. All birds fly. "Immediate association of birds and flying: replied 'Yes' without thinking. Then a 'pulling back feeling' in mouth and throat. Image of an ostrich, with the *idea* 'This bird does not fly.'"

In spite, therefore, of the defects of the method we have some evidence of the existence of a definite consciousness of negation, though we cannot state how frequently it occurs. As in the arithmetical tests, it is described in terms which suggest that it is quasi-emotional in character, and it frequently contains kinaesthetic constituents. It would seem that this experience is not precisely the same in all cases. Its exact constitution depends on factors which cannot be fully analysed at present. The central core is an attitude of hostility or, its weaker form, mistrust. The kinaesthetic phenomena can usually be explained as the remains of the gestures which evidence these attitudes in more practical cases. The existence of this conscious attitude is not necessarily dependent upon analysis of the presented statement, and, consequently, does not demand that a true judgment should be formed before denying the one presented. But the occurrence of the consciousness of negation (and immediate denial) is more rare when the stimulus is verbal than when it is numerical. Why this should be so is discussed in the concluding section.

VI.

Negatives of Denial: (*c*) *Series* 5. *To judge the correctness of a picture.*

The subject was instructed as follows:

> "You are to say whether these pictures are right or wrong. You are to ignore bad workmanship. Errors will be definite misrepresentations of some important fact."

The ten pictures used were sketched in water colour on cards. The subjects were an oak-tree, a giraffe with a cock's head, a ship with masts and funnel raking towards the bows and flags streaming in opposite directions while the smoke ascended vertically, an axe, a cottage, a cutter-rigged yacht, a horse without hind-legs, a jug with an inverted handle, a fish, and a cat with a black face with its body striped blue and red. These were exposed in a tachistoscope for a quarter of a second. There seemed little to choose between this and a continued exposure, but on the whole the writer thought it better to remove as far as possible the chance of subsequent analysis or judgment, thus reducing the introspective task to the slightest. This form of experiment was contemplated at the outset of the enquiry and then laid aside. The results of the other series pointed to the necessity of simple and striking material such as this, but it was then too late to make a series of the same length as the others. The pictures had to be sketched by the experimenter, who is far from being an artist. The results of these tests consequently only serve to illustrate points already mentioned. As *D* missed this series there are only 60 protocols, of which 27 give the object as wrong. After a most rigorous scrutiny it appears that in 18 cases the judgment occurs without any consciousness of what the picture should be. It is immediately classed as wrong, either generally, or in some particular point. Here, then, the negative of denial is independent of an affirmative process. In thirteen cases reference is made to 'feeling,' which is more frequently particularised than in the other similar tests.

(*a*) *Disappointment.*

A. *Giraffe*. "Feeling of disappointment, and then amusement."

A. *Horse*. "Animal of some kind: followed it back, and white seemed more intense where legs should be. A feeling of disappointment, and a faint image of what thing should look like."

(*b*) *Absurdity.*

B. *Giraffe*. "Got the picture. An appreciable time afterwards analysed it into two parts. Lower part gave impression of giraffe. Then remembered cock's head, which seemed absurd and strange. This caused answer."

C. *Cat*. "Nothing in way of judgment at all. First thing I was conscious of was beginning to laugh, before seeing what was wrong."

M. *Giraffe*. "Feeling of ludicrousness. No thought of what picture should be."

(*c*) *Strangeness.*

C. *Horse*. "Immediate recognition of horse, and awareness of something wrong came afterwards, a strangeness. Analysis of image brought awareness of incompleteness."

(*d*) *Amusement.*

N. *Cottage*. "Faint feeling of amusement, of something strange, as of object suspended in air, and not attached to the ground."

In these instances, then, the mental concomitant of the response is emotional in character, and this feeling frequently arises without any imaginal or ideal representation of what the object should be, and sometimes without a clear apprehension of the object itself.

VII.

We may now attempt to formulate some general conclusions from the whole series of experiments. The validity of these conclusions will, of course, be limited by the range of data available, and for a large part we must be content to leave the matter as hypothetical. But one point seems perfectly clear, that the two forms of negation which have been styled Negatives of Construction and Negatives of Denial are psychologically quite distinct. It may be objected that the evidence is rather artificial, inasmuch as this distinction was dependent on the instruction, so that we obtained nothing more from the experiments than was originally put in by the experimenter. This objection will not hold, for, as we have seen, the observers sometimes break away from the instruction into processes which are not of the type sought in that particular series. But these spontaneous processes nevertheless fall into these main types, and show the same characteristics as those most evidently dependent on the instruction. Thus we have seen that a constructive judgment may contain a denial as part of the whole process, while in many cases the process takes the constructive form when the instruction is directed towards denial. These cases are sufficient to show that the difference found between the two types is not merely the consequence of the experimental procedure, but is a real difference in mental activity. While it is true that in the majority of our examples the type is determined by the methods adopted, this is only because the methods are conformable to existing differences.

Summarising the results obtained in Sections 2 and 3, we see that the Negative of Construction differs from the corresponding affirmative type in verbal expression only. For logical theory verbal propositions may require to be divided into affirmative and negative, but there is no psychological difference between the judgments underlying them. Whether the resulting proposition is to be affirmative or negative, the course of the judgment is the same. It is determined throughout by the thinker's purpose. If this demands that the last moment should be an association by contrast, there is still nothing essentially negative about the process, especially if we hold that association by contrast is (in most cases at least) reducible to contiguity. But if it is admitted that the experiments give no reason for distinguishing constructive judgments as affirmative and negative, it may still be questioned whether this result is due to a failure in the method, or whether it is true of normal processes. The writer would maintain that the method reproduces as exactly as possible the actual course of judgment. For all thought is controlled by previous thought, in every case the thinker has some purpose in view, and so every judgment is a mental construction designed to meet the demands of the moment. In our experiments this control is replaced by the 'task,' and though the latter is in some measure necessarily artificial, yet we have a sufficiently close approximation to the thought process which arises more normally. There is no need to labour the point at this date, but if it be accepted, then it follows that judgments are not distinguishable as negative and affirmative so far as the form of the process is concerned. Neither can we find any additional determinant. In these examples there is no consciousness of negation described. The large number of such references in the other series precludes us from ascribing this to the incompetence of the observers. But if this feature is lacking there is no possible way of introducing the distinction of quality into the process, for the frequency of the cases in which the response is mediated by another judgment gives no ground for such a distinction. And if the figures were at all conclusive it would be very difficult to translate them into a psychological classification. Our conclusion, then, is that the judgment usually symbolised by the schema *S is not P* is more adequately represented by the obverse form *S is non-P*. The term *non-P* is no mere negative. It is a definite idea which can be represented by a positive name, or at least has some positive significance for the thinker. Thus *S is not P* is in most cases nothing more than a verbal equivalent for *S is X*.

The process leading to the verbal denial of a proposition differs from the parallel process of agreement and from the constructive judgment. Except in the most trivial cases the negative of construction demands the prior formation of a positive judgment, but the denial of a proposition may quite well take place without any previous analysis. This tended to be the normal form in the numerical examples and was frequent in the verbal series: in the incomplete perceptual series, too, immediate denial was extremely frequent. But if immediate denial can take place at all in really significant propositions, its frequency is of secondary importance. However few such cases might be, they would show that negation in this sense does not depend upon affirmation, except in the obvious sense that there must be something to deny. I may deny that *S* is, or is not, *P*, without having in mind at all what *S* is. Negation, therefore, can be aroused immediately. Further, it has a psychological character which distinguishes it from affirmation and from construction. It is a definite experience, existing side by side with the content denied. We have insufficient data for describing it in detail, but there can be no doubt of its existence, nor of the fact that it is emotional in character. It is variously described as surprise, disappointment, etc. It contains kinaesthetic and other sensorial factors, which are variable. But however much its composition may vary it is essentially a mental disturbance, an attitude of caution or hostility in face of the unexpected. The corresponding consciousness of agreement is less marked, but it exists, and, like negation, may include kinaesthetic elements, such as a "mental nodding of the head." Of these points there seems to be no doubt. We conclude, then, that the consciousness of negation is a complex attitude, including emotion and sensorial factors, and that it is found only in the case of denial.

Mental behaviour is secondary to practical bodily action. Not only does it develop later in time, it also develops under the stress of practical needs. In its most highly developed forms it still tends to show evidences of its origin, and in animal behaviour we may find many of the phenomena which characterise the intellectual sentiments. So we may find the original denial in such examples as that of the dog robbed of a bone. Hostile action in resistance to interference is the most definite form of denial that can be found. In other cases this breaks down into the mere attitude of hostility, or of mistrustful caution. Here the action is only incipient, the muscles are partially innervated and the concomitant emotions are aroused, though, perhaps, in weaker form. The emotion and kinaesthesis together make up a very definite and intense experience. Now, the protocols we have dealt with suggest that the consciousness of negation is the weakened form of this attitude. *E*, for example, speaks more than once of "indignation" in denying a statement, *D* of "a feeling as if brushing the whole thing aside," and the note of a casual introspection of my own illustrates the same point. While thinking of the nature of thought processes I wrote the words, "They are in no respect like logic," and at once made an introspective note of the process immediately preceding the writing. It consisted of an "auditory image of the word 'logic,' a visual image of a grey amorphous background on which was printed in white letters LOG, and an experience of strain in the back of the neck, whether sensational or imaginal I cannot say." I find that in my own case this strain is a fairly regular accompaniment of negation, and it appears also in some of the protocols quoted. Now the same experience occurs in the attitude of 'taking guard,' and consequently it is at least possible that the consciousness of negation here includes part of a combative attitude. In other cases we have references to 'contempt' and 'absurdity' which are more difficult to derive. But even these emotional attitudes in extreme cases include hostility, and there is always the appropriate gesture with its accompanying kinaesthesis. Taking all these facts into account, remembering, too, the intense anger which may be aroused when we hear a statement that is, in our opinion, outrageously false, it is not too fanciful to see in this consciousness of negation the degenerate vestiges of that primitive, yet final, form of negation, the *argumentum ad baculum*. The outward expression is largely lost, and there remains only a flicker of emotion and kinaesthesis. But in essence it seems the same.

It may be open to question whether this process of negation is properly to be called a judgment. This is in any case a question of quite secondary importance. The process exists and has to be dealt with: the question where it is to go in any scheme of classification is interesting but not urgent. We know the difficulties met with in the search for a psychological definition of judgment, but they have not greatly hindered the study of the thought processes. So it seems sufficient to say that this process of negation is as much a judgment as any other; it is a highly self-contained process commencing with the apprehension of the stimulus and proceeding, possibly through the mediation of analysis, to its final term,—the consciousness of negation.

The consciousness of negation may be considered from another point of view. It is emotional in character, and emotion is correlated with the excitement or disturbance of some interest or 'set.' The nature of the protocols shows that it is here a question of disturbance. The consciousness of negation arises through the interruption of a mental set, which is determined by the organised system of knowledge of the thinker. The stimulus is apprehended serially, the first term arouses expectant tendencies of a general character, or a sub-excitement of acquired dispositions. If the rest of the stimulus is consonant with these tendencies, we have what one observer calls "a comfortable feeling of agreement," or "a mental nodding of the head." But if the predicate runs counter to this set, the shock arouses emotion, and that without any review of our positive knowledge. We are simply aware of an object as thwarting our prejudices. The more completely organised our mental systems are, the more readily is our emotion excited by contradiction. Here we find the reason for the difference in the proportion of immediate negatives in Tables III and IV. No knowledge is so completely organised as that of elementary arithmetic, and the material we employed was homogeneous throughout. But the verbal propositions cover a large number of spheres, which cannot all be familiar to every observer. We obtain immediate judgments, therefore, in only a few cases, where the observer's system of ideas is quite definite, though not necessarily correct.

In other cases (in which the thinker is not, as it were, so *personally* concerned) analysis of the stimulus is demanded, and possibly a review of his knowledge on the subject. The mental set is incomplete and needs hardening by conscious processes. This form of negation ends in one of two ways. The conscious reinstatement of thought contents may be sufficient to constitute a set, in which case the denial occurs in a way closely similar to the immediate response. If, on the other hand, knowledge is relatively incomplete, a new constructive judgment is formed. Thus, confronted by the proposition *S is P*, the subject analyses his knowledge of *S* and forms the judgment *S is X*, *and therefore non-P*, and answers "No." But for him "No" is not a denial, but a verbal translation of his judgment *S is non-P*, the translation being made under the control of the experimenter's instruction. In these cases the process reverts to the constructive type and the consciousness of negation vanishes. We have already noticed the complementary case in which a denial occurs within a constructive process. The stimulus is here internal, being a construction depending upon a partial and incomplete mental system, but the psychological character of the negation is the same as if the stimulus had been presented by another person.

From the above analysis it is evident that the two forms of negation cannot be reduced to one. For the purposes of logical theory it may be necessary to take the propositions "*S* is not *P*" and "It is false that *S* is *P*" as equivalent, but psychologically the judgments expressed by those schemata are quite distinct. And it must be remembered that the verbal form of the proposition affords little clue to the nature of the judgment, since the purpose of communication is, as we have seen in the protocols, something over and above the actual judgment. In normal thought the two forms are inextricably woven, but they remain different. The consciousness of negation arises only in face of a challenge: the constructive judgment knows nothing of affirmation or negation,—it is construction and nothing more.

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A FURTHER NOTE ON THE SENSORY CHARACTER OF BLACK.

BY JAMES WARD.

IN the first volume of this *Journal*[1] I raised anew the question, formerly often mooted, Is 'Black' a sensation? The evidence that it is seems nowadays so conclusive to the majority of psychologists and physiologists that anyone who advances reasons for thinking otherwise is classed among the circle-squarers, inventors of perpetual motions and other deluded cranks. No wonder then that for more than ten years my article was ignored. Yet now at last my old friend, Professor Titchener, has vouchsafed to give me the *coup de grâce*[2].

But the Professor has been too merciful. After summarizing the psychological evidence, he remarks: "We should all, nevertheless, welcome an *experimentum crucis.*" After all, then, the case is not yet quite a *chose jugée*. Nevertheless Professor Titchener thinks he can maintain (i) that no further psychological evidence is necessary, and (ii) that my 'review of visual theories' is really irrelevant to the main question.

(i) As to the summary of the psychological evidence which he regards as conclusive, I will venture at this stage on only two remarks.

(*a*) This summary, as it stands, reminds one of certain exciting trials in which the circumstantial evidence against the prisoner appears to be overwhelming, when lo! an *alibi* is forthcoming that alone suffices to dissipate it all. If black verily *is* a positive entity it may account for the facts marshalled to prove it; but there is one simple consideration that militates against this assumption, even if it does not completely overthrow it. Experience cannot begin with negation and vision cannot begin with black. This Hering fully admits: "the sensations of white and black," he says, "both alike are due to the influence of objective light, *only with this difference that the white sensation* [*sic*] *is developed under the direct, but the black under the indirect, influence of the light-stimulus*[1]."

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[1] pp. 407–27.

[2] "A Note on the Sensory Character of Black," *Journal of Philosophy, Psychology, etc.*, March 2, 1916, pp. 113–21.

How Hering explains this indirect action of light I shall refer to later on. Meanwhile that he allows the sensation of black to presuppose that of light is enough for the present. The sensory character of black is allowed to be so far unique. This point is, in my opinion, essential to the case for the negative quality of black; and though it may not be sufficient alone, as I have already said, the case would be hopeless without it. Positive sensations, I have argued (p. 409), can occur in any order; not so negative. Among these, then, *prima facie*, at all events is the place of black. Professor Titchener, however, soon disposes of this crucial point to his own satisfaction, so that 'further argument,' he thinks, is needless. Unfortunately he has, I fear, been misled by my final summary (p. 426), where blue and green—selected for quite other reasons—happen to be cited as being—unlike white and black—independent of the order of presentation. At any rate he supposes that I am 'speaking genetically' and so far 'speculatively.' He therefore thinks it pertinent to reply: "Blue, I take it, appeared earlier than green; and black and white appeared together" (p. 119). Genetically they did, no doubt: a fish that can see light does not have to wait for further differentiation of its light-sense before it can see black as it must do before it can see colours. All that is needed is the cessation of the objective light. My point, in short, is the obvious one that the experience of darkness presupposes the experience of light and answers to its absence. This and this only is the '*alibi*' that bids fair to save black from all its troubles. So completely does Professor Titchener miss my point that he says: "Ward too assumes that the primitive sensation of sight consists of a 'light' which ranges from complete darkness to dazzling brightness." What I actually said was "consists of the *single* quality we call 'light,' a quality which ranges in intensity from the *zero* of complete darkness, etc." (p. 408).

(*b*) "There is no ground for separating the blacks from the darks," Professor Titchener says in bringing his brief summary to a conclusion. It is this statement that leads me to my second remark. It would be strange that two terms should exist and have their equivalents in so many languages, if all the world were, like Professor Titchener, unable 'to distinguish between "complete darkness" and black.' The perception of such darkness implies 'an unlit space'—a dark-field, the perception of black on the other hand, is possible in a light-field. According to Hering, in fact, who also originally identified black and dark, the deepest black is possible only there, the darkness of an unlit space not being completely black[1]. Further, Hering now recognises various 'darks,' regarding blue and green as having each an *Eigendunkeln*, but, unlike Professor Titchener (p. 113), he recognises only one proper black—an ideal to which the nearest approach is made by contrast in the light-field[2].

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[1] *Zur Lehre vom Lichtsinn*, S. A. 1874, p. 61. Italics mine.

The language of everyday life agrees with this. For it there are various darks, always implying 'shade,' i.e. either diminished luminosity or diminished illumination; but there is only one true black, predicated not *of* colours but of things or forms as their colour. This black, like Hering's, is only perceptible in the light-field. This fact, *viz.* that there are occasions in which it is appropriate to speak of dark and others in which it is only appropriate to speak of black, is really the mainstay of Hering and his school. If then black never appeared in the light-field their case would be desperate[3]. It is certain that we never perceive form without content, nor visible form without some colour. In this respect black is on a par with the rest. For all that it does not certainly follow but black itself is a positive sensation; though a 'body-colour,' i.e. a secondary quality in the epistemological sense, we must allow it to be. Blackness and darkness are then *prima facie* distinct. In the last resort possibly one may be resolved into the other; but it will make all the difference which. If 'black' can be resolved into 'dark', it will, I repeat, be difficult to prove that it is a colour. Even if 'dark' be resolved into an approximation to black, there is still much to do.

How on this supposition is the question to be settled? By direct appeal to psychological observation, Professor Titchener thinks. This, I believe, will go a long way towards establishing the privative character of the darkness which befalls all colours alike; but does direct observation disprove the positive character of black? Black, we say, may answer to a portion of the light-field that is devoid of light. But we have to allow that at any rate form is just as readily perceived when it is black on a white ground as it is when white on a black one. Whereupon the attempt is made to turn the tables upon us. We have pressed the logical difference between privation and negation[1]: the retort is to insist on that between negative and opposite. Black is not negative and therefore not privative: it is the opposite of white. Black and white are related much as land and water, island and lake. Very well, we rejoin, then they must be equally independent and black must have a positive stimulus and one as easily assigned as is that of white.

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[1] Cf. *Lichtsinn*, p. 63.

[2] Cf. *Lichtsinn*, § 24 and "Grundzüge der Lehre vom Lichtsinn," *Graefe-Saemisch Handbuch*, III (1905), pp. 60 f. This change of view is due to his theory of 'specific brightnesses.'

[3] I may be allowed here to repeat from my original article two quotations from v. Kries: (*a*) "The unbiassed will always be inclined...[when the eyes are closed] to speak of a cessation of seeing, an absence of sensation (*Nichtempfinden*)." On the other hand, (*b*) "unbiassed introspection will never allow black to count as a lower degree of intensity of the sensation of white" (p. 411).

(ii) To this demand, however, as I have already mentioned, Professor Titchener demurs: it raises an issue that is, he holds, irrelevant to psychology. And he imagines that he has Hering's support in so restricting our question to the strictly psychological domain and abjuring all physiological incursions. But if physiologists who abjure psychology make statements that are psychologically barbarous or an affront to common sense, have we no right to intervene? As a matter of fact their avowed standpoint is psychophysical, or rather physicopsychical: psychology, as Professor Titchener, I believe, conceives it, they repudiate altogether[2]. But whether psychical facts are to be dubbed phenomenal or epiphenomenal our right to ask questions is surely indisputable. As to the particular question I have raised—its relevancy is admitted by everyone of the physiologists concerned. They begin by laying down psychophysical principles to which they appeal[3]. To every *psychosis* there is a causal or at least a corresponding *neurosis*, is the chief of these. We ask them for the *neurosis* answering to black. For all other visual sensations the physical stimulus is obvious—aetherial vibrations of some kind; but there is such direct stimulus for dark or black. If there is a stimulus at all it must be an internal state or change of some sort in the visual apparatus itself, retinal or central or both. So far all seem to be agreed, but as to the nature of this state or change there are two radically different views, that of Helmholtz—the resting state of the organ; and that of Hering

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[1] "Privation, so far as consciousness is concerned, is nothing else than absence," Professor Titchener maintains. The relevance to our question of the paragraph in which this *dictum* occurs I have failed to see and am uncertain therefore with what limitations, if any, this striking deliverance is to be understood.

[2] Cf. Hering, *Lichtsinn*, p. 2.

[3] Cf. Hering, Mach, v. Kries, G. E. Müller.

—a restorative or assimilative process within it. The first will not account for a specific sensation, the second might, if it were not wholly problematic and inherently improbable.

It was against Helmholtz's view—that the resting state of the organ yields an actual sensation—that I urged the analogy between darkness and silence[1]. This Professor Titchener oddly enough takes to be argument on which I chiefly rely and he devotes to it much more space than I did: he even seems to have made a number of experiments to dispose of it. But I have reluctantly to confess that the drift of much of what he has said is beyond me; I will however try to deal with some of his points.

(1) In a note (p. 410) I had spoken of 'a silence that could be heard,' and my critic is at pains to show that the proper expression would have been 'a silence that could be felt.' My remark obviously was not concerned with sensations at all, and therefore not with the various organic, kinaesthetic or other sensations that may under certain circumstances accompany the cessation of sound. What may be *felt* seems nothing to the point. All sorts of changes, sensory and emotional, all sorts of associations may accompany the cessation of light but nobody has argued that these help in the remotest degree to determine the sensory character of black. Similar concomitants seem as foreign to the like question in the case of silence. Macaulay ridiculed those who defended Charles I against the charge of being a bad king by proving that he was a good husband and a good father, a perfect gentleman, and so on. I should not like to say that my colleague at Cornell displays a similar *ignoratio elenchi*. I prefer to say that, with one exception[2], I fail to see how his experiments and observations bear upon my point.

(2) My point was this: the cases in which we might refer to the cessation of sound as audible are—like 'the silence in heaven for the space of half-an-hour'—cases of *perception*, where "consciousness, when

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[1] Urged long before by Fick, as Professor Titchener reminds me. I knew that well enough and I have twice referred to Fick. To the article in Hermann's *Handbuch* I should have referred too, had it been mentioned by Stumpf, with whose endeavour to disprove the analogy I was immediately concerned.

[2] The observation that 'entotic phenomena' bar the way to 'the realisation of an auditory blank even in an objectively quiet place' (p. 115). To this there is the parallel of the entoptic phenomena (Helmholtz's *Eigenlicht*) in the case of objective darkness. The absolute ideal is unattainable either way. But at all events the blank in both cases is complete enough, as Fechner said (*In Sachen der Psychophysik*, p. 127), to put an end to the paradox that silence or blackness, if it were absolute, would, as such, yield us any positive sensation.

there is nothing to hear, may still be auditory," as Professor Titchener himself has said (p. 116 *fin.*). But darkness and black in vision seem analogous to silence and pauses in sound, why then should the absence of positive sensation, generally conceded in the latter case, be disallowed in the former? This question, I feel, Professor Titchener has not directly faced. Although he admits 'my effort to distinguish between sensation and perception to be wholly justified' in itself (p. 120 *n.*) he dismisses it here as 'only a Podsnappian flourish' (p. 115 *fin.*), because it ignores 'the various organic stimuli,' etc., that may be also in play. For lack of understanding, it may be, I am unable at present to concede this.

(3) Whereas sight is now preeminently the spatial sense, hearing is preeminently the temporal one, and as pauses therefore are for hearing (perceptually) auditory blanks so, I have contended, black may be regarded as, for sight, (perceptually) a visual blank. In illustration I referred to the parallel drawn by Preyer between the use of black in painting and the use of pauses in music, and again to the parallel drawn by Mach and v. Ehrenfals between 'time-shapes' and space-shapes (*Gestaltqualitäten*). As to the former, Professor Titchener complains that it is 'unhappily worded': I agree. However the analogy, I am disposed to think, still holds good[1]. Chiaroscuro in painting answers to the crescendos and diminuendos of music and the limit of blackness in the one seems on a par with that of stillness in the other.

Anyhow the parallel between time-shapes and space-shapes is more instructive and Professor Titchener's criticism less effective. He begins by pointing out that according to Mach "there is a peculiar, *specific time-sensation.*" No doubt, 'according to Mach'; and there is a peculiar, specific space-sensation too, for that matter. But much as we owe to Mach, few, I imagine, would say that in elucidating the distinction between sensation and perception he has helped us at all. Yet Professor Titchener thinks otherwise; and accordingly he tries to refute me by further testimony from an authority to whom he supposes I had myself appealed. But no: shape qualities, temporal and spatial relations are not for me sensations but 'objects of a higher order,' as Meinong called them. It is useless, then, for Professor Titchener to turn upon me by saying that "the alternation of sound and silence is for Mach an alternation, not of sound and nothing, but rather, of sound-time and of pure (sensory) time; the 'blank' interval has a specific time content." I never imagined that the blanks whether in sight or sound were absolutely nothing at all. They are perceived as forms and admit of measurement. An absolute blank for consciousness is clearly impossible: some specific time content there must be whenever we are aware of silence but that does not prevent silence itself being an auditory blank.

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[1] Against Professor Titchener's *dictum* that 'black is not indispensable to the painter's palette,' I may set Leonardo da Vinci's that it is.

Against Hering, however, to urge the analogy between silence and darkness would be wasted labour till his arguments for the positive character of black, based on the facts of light-induction and contrast, had been met. My attempt to deal with these (pp. 413 f.), Professor Titchener has found 'very puzzling.' I do not think the fault is altogether his, and I must now try, while correcting some of his misapprehensions, to make my own meaning clearer.

Really the first point was to fix the meaning of contrast, but I left it till almost the last (p. 414 *med.*). "To call a difference of intensity 'a contrast,'" says Professor Titchener, "simply begs the question." I prefer to say that it confounds two quite distinct things—a mere difference which may exist without comparison and a 'contrast' which cannot. When two intensities differ, it is possible to compare them, and if they differ greatly, to contrast them, as we constantly contrast extreme differences of degree of all sorts—*e.g.* great and small in respect of size, rich and poor in respect of wealth, weak and strong in respect of strength, &c. But to assume that 'contrast' is given then and there, whenever two intensities are both positive and of opposite quality is, I maintain, equally to beg the question; or rather it is again to confound two entirely different things—opposition or antagonism—which can exist without comparison and 'contrast' which cannot. Yet that is the assumption which Hering makes. But contrast, as such, does not imply opposition. As the common use of the algebraic symbols + and − shews, opposition is disclosed by neutralisation or counteraction. It is to this fact that Hering appeals in ranging red and green, yellow and blue as *Gegenfarben*—a criterion which fails in the case of white and black. Contrast, I have said, is primarily a psychological concept, and *strictly* speaking—I will venture to add—its domain is always that of a linear scale of some sort. The zero point of this may be one of the extremes, as seems to be the case with black and white; or it may lie midway between them as in the case of red and green.

Deferring for the present the application of all this to the case of sound, I come now to the fact (or hypothesis)[1] of 'negative induction' as a physiological process accounting for the familiar darkening of the gray strip on a white ground. That there is no analogous process between a louder and a fainter sound I was well aware. But if darkness is negative, a 'negative' process which renders a given gray darker surely does not thereby tend to prove that darkness is positive. What was dark—the so-called *Augenschwarz*—being matched to a body-colour appears, when contrasted with white, to be darker still. However produced the one psychological result is just this increased darkness.

"*Leaving physiological differences aside*," I then continued, "we can at least imagine a parallel in the case of sound, which would hold good psychologically at all events. Suppose we hear a loud sound of constant intensity and quality, and simultaneously a much fainter one of different quality[2]. Let the intensity of the latter steadily diminish and the contrast in intensity between the two sounds will steadily increase; and there is *a priori* no reason why we should not attribute this growing contrast to the increasing stillness of the waning sound" (p. 413). But my critic sees no parallel: "the gray on the white ground does not grow continuously darker; on the contrary it gradually lightens." Quite true, and the objection is a perfectly natural one; and yet a little more care on my part might have obviated it. I regarded the faint sound as corresponding to the gray, certainly; but I conceived the intensity of this sound steadily waning, while the louder sound continued constant, to be the parallel situation, *as regards contrast*, to that of the constant gray in light-fields of increasing intensity. The objective variants and the constants, so to say, change places but there is a 'growing contrast' in both cases alike: in the increasing black owing to negative induction[3], in the increasing stillness owing to the waning sound. The limit of such increasing stillness is an 'auditory blank' and the 'deepest black,' the limit of the visual series, Hering himself represents by zero (0). So much by way of clearing up Professor Titchener's misunderstandings. He repeats in this connexion his objection that an auditory blank has other 'sensory content' with which I have already tried to deal.

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[1] There are difficulties in the way of Hering's position which I neglected to mention in my former paper. Cf the masterly article by von Kries (Nagel's *Handbuch*, III, pp. 243–5).

[2] "The two sounds, heard together, if tones, would assuredly either blend or beat," Professor Titchener objects. Not necessarily, I believe, for perception; inasmuch as there are limits to the perceptibility of beats. Anyhow 'sounds' is the word used, not 'tones.'

[3] Hering himself gives figures illustrating this. Cf *Grundzuge*, p. 116.

There is one criticism, it is the first that Professor Titchener makes, which I must admit to be—*au pied de la lettre*—effective. I had said that "in a light-field many colours may be distinguished, but in a dark field none." My critic reminds me that provided the light-field is light enough no colours can be distinguished in it; implying, of course, *per contra* that unless the dark-field is dark enough, colours can be distinguished in it. There is no denying this as ideally true. Practically we can only say with Hegel, that in the dark all cows are black. But theoretically it is just as true to say that in the light all cows are white. White then corresponds to one apex of our colour octahedron and black to the other. The interpretation of this superficial symmetry is however the important problem: it is in fact the very question now under discussion. If we regard the main axis of the colour manifold as symbolizing a simple intensive series ranging between a threshold value 0 and a lintel value 1 the disappearance of colours at the lower limit is a logical necessity. Their disappearance at the upper is just an empirical fact that might conceivably have been otherwise. Albeit it is an instance of a wider psychological generalisation, that "presentations of medium intensity are clearest[1]." Also the fact that the colour seen is a genetic differentiation of the light-sense helps to explain it.

The alternative is to regard black and white as, like red and green, equally positive and mutually antagonistic—the view, of course, of Hering which Professor Titchener defends. A blinding and dazzling black then becomes as truly a possibility as blinding and dazzling white is certainly a fact. "But whoever heard of a *blinding* black?" I asked, meaning of course a black so intense as to be painful like excessive light. Our eyelids are a protection against blinding white but what protection should we have against blinding black? Fortunately, as Hering assures us, there is really no such black[2]. Professor Titchener, however, has thought it pertinent to meet my question by quoting what Ebbinghaus has said—*à propos* of 'general adaptation'—about *impenetrable* blackness[3]. But my question referred not to adaptation but simply to sensation. To say that whereas a sensation of light may be blinding a sensation of black may be 'impenetrable' seems curiously awry. To me it seems to be as much off the lines as Professor Titchener's handling

[1] Cf. Ebbinghaus, *Grundzüge der Psychologie*, 3rd ed. I. p. 752.

[2] *Lichtsinn*, p. 89.

[3] *Op. cit.* I, p. 247

of silence as not an auditory blank because it has non-auditory accompaniments.

I have mentioned five 'difficulties' that Hering's theory entails (pp. 414–7) yet this is the only one with which my estimable colleague has attempted to deal. Considering the minuteness of his criticism of smaller points this is surprising and disappointing. His contention is that 'visual theories' are irrelevant and he prefers a short and easy method by way of psychological observation. Yet three of the remaining difficulties I have mentioned as besetting Hering's theory turn on points of psychology as do also many of my strictures on Wundt. His summary ruling-out of all this part of my discussion (p. 113) certainly does not suggest, and is not likely to beget, confidence in the strength of his case.

I made no rejoinder to Wundt's reply to my paper to which Professor Titchener refers. I must say that I did not think it very straightforward. When I complained of his habit of *silently* changing his opinions without admitting his previous mistakes he affected to believe that I twitted him for having the courage to live and learn. He felt exonerated from discussing my criticisms because they were essentially of an abstract and conceptual nature, and his one domain had always been experience! In a short final paragraph however he decides to ask me just *one* question. If black is not a sensation but the absence of a sensation why don't the totally blinded see black? What is more, he obligingly answers the question himself: *Schwarzempfinden und Nichtempfinden sind zweierlei*: in other words, the privative and the negative are not the same! So I had already said, though Professor Titchener does not agree.

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AN EXPERIMENTAL STUDY OF SOME PROBLEMS OF PERCEIVING AND IMAGING.

By F. C. BARTLETT.

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1. *General introduction.*
2. *Method of experiment.*
3 *Description of experiments and of results:*
    (i) *Series* 1, *Simple diagrams and designs.*
        (*a*) *Group* 1,
        (*b*) *Group* 2,
        (*c*) *Group* 3.
    (ii) *Series* 2, *Kinematographic material.*
    (iii) *Series* 3, *Simple concrete representations.*
    (iv) *Series* 4, *Complex picture material.*
4. *Summary of results from Series* 1–4.
    (v) *Series* 5, *Ink-blots and ambiguous outlines.*
5. *Discussion of results—Complexity of process of perceiving—'Effort after meaning'—Feeling and imaging—Changes in content of imaging—'Feelings of relation' and the beginning of analysis—Close relation of perceiving, imaging and thinking.*
6. *Summary.*

[At the outset I wish to express my indebtedness to those who have helped me in the preparation of this paper: to Lieut.-Col. C. S. Myers, R.A.M.C., who suggested the general subject of the investigation, afforded facilities for the carrying out of the experiments, and willingly gave valuable guidance throughout; to Prof. James Ward, who gave many suggestions with regard to the methods of experimentation, and with respect to authorities to be consulted; to Miss E. M. Smith and Dr W. H. R. Rivers, who read through the paper in typoscript and made a number of valuable criticisms; and to the many subjects who willingly submitted to the tests. What value the paper may possess it owes largely to the assistance that has been given in these various ways.]

1. GENERAL INTRODUCTION.

It has often been pointed out that "perceiving is an act, a thing that we do, never a mere passive sensing of a group of passing sensations or impressions[1]." In the course of development this act may become very complex, so that a study of the factors that determine its nature may involve a reference to a number of processes that have been sorted out and named by psychologists. But always, wherever there is perceiving, there is a direction of attention upon an object that is to be regarded as being actually present.

This statement may itself, of course, give rise to various disputes. Is it really necessary that the object reference should be clearly made before what we can call perceiving may take place? What does "being actually present" mean? Can we find when and how a subject says of something distinguished from his own act of apprehension that it is 'here' and 'now'? These are difficulties which even the simplest statement of what is ordinarily meant by perceiving may raise.

But with these the present study has nothing to do. Its first part is concerned solely with ways in which adult subjects set about perceiving material of greater or less complexity that is presented to their observation, and with the factors determining these ways. The consequent limitations of the experiments have to be kept in mind throughout.

First nearly all the subjects examined were adults, and it has to be remembered that these brought to their task habits of observation that had already been formed by long process of development, and often set about the problems given to them under the influence of general attitudes which had been determined by much earlier experience. This all made analysis more difficult, and in particular it was necessary always to guard against the tendency on the part of such subjects to treat processes which could occur only as a result of considerable training as themselves simple and fundamental.

'Simple,' in fact, is a term that may have several different shades of meaning. In these experiments there is a simplicity of task, a simplicity of material presented, and a simplicity of factors present in the act of perceiving itself. Generally the term was applied to a task which, because of its familiarity or for some other reason, a subject found to be easy. Sometimes it was used of material which contained little detail. Again the term may be employed to indicate factors which appear to be constant throughout a whole series of experiments, and so to be required for all cases. It is often used also of factors that are definitely regarded as not capable of further analysis, as foundational in a historical sense, as original elements of the mental life. These different meanings may be readily confused; and particularly when a task set is easy it may seem as if the distinguishable factors involved are incapable of being further analysed. But in this respect ordinary introspection, as Professor Dawes Hicks[1] has often pointed out, is liable to go much astray. A subject's judgment: "This is simple," must always be received critically. And in fact it cannot be claimed that experiments of the kind here described of themselves throw much light on the question of what is to be regarded as simple in a foundational sense.

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[1] Huey: *The Psychology and Pedagogy of Reading*, p. 104.

Then also the inquiry into the various ways in which we set about perceiving was limited by the fact that the same sort of problem was put throughout. No doubt what Goldscheider and Müller found in the case of reading holds good generally. They said that when we read we perceive in various ways according as our purpose may be best secured. But throughout these experiments the nature of the problem set was constant. The question was simply one of giving an accurate reproduction of presented material. Changes of method therefore, when they occurred, were due to other factors than that of change of purpose.

Again probably the definite effort to reproduce accurately may have meant the adoption of methods and attitudes somewhat different from those of ordinary observation. This difficulty was of course unavoidable from the very nature of the experiments, but it had to be borne in mind throughout, and the results of the experiments checked, as far as possible, by reference to common experience.

The general problem therefore was to attempt to carry through a study of ways of perceiving, and of the factors influencing those ways and their results. It soon became evident that this would involve a study of the nature of imaging, for the two processes are commonly found together in a single act of observation, although they are of course to be discriminated.

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[1] See, for instance, his article on "The Nature and Development of Attention": this *Journal*, VI. 7:—"It has to be borne in mind that what may appear to us introspectively, in our mature experience, to be a simple and unanalysable process, need not by any means necessarily be a simple and unanalysable process as a psychological fact."

In perceiving there is always direction of attention upon an actually present object. It is always the object or some part of it that is known, but it is known, not in a literal sense immediately, but by means of present sensuous experience of some sort. Even in the simplest act of perceiving this sensuous experience is not treated as complete in itself, but as referring to 'something' which in later development gains for us all the marks of object[1]. In his description or representation of a given object however a subject, even from very early stages, frequently passes beyond what present sensuous experience justifies, and brings in characteristics which a more careful observation will show him do not belong to the object presented. Whenever that occurs there is imaging, though of course the subject at the time thinks that he is describing what is actually present to his observation. It is in this sense that imaging came into the experiments from the beginning, and not of course in the sense in which imaging means a characterisation of an object which at the time of the reference is regarded by the subject himself as not present to his sensuous experience[2].

2. METHOD OF EXPERIMENT.

The method adopted was as follows. Material was presented to the subjects, and they were asked to say what they had seen, or, in the case of the final series, what they had been reminded of, or had imaged. The material was graded in nature, and passed from simple designs and patterns to designs the structural complexity of which was considerably greater, to concrete picture material, and eventually to ambiguous outlines which were used in the study of processes of imaging. The presentation was made, in the case of some of the picture

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[1] Cp. Stout: *Manual of Psychology*, 3rd edit., p. 132: "We must therefore assume that the simplest datum of sense perception from which cognition of an external world can develop consists, not merely in a sensuous presentation, but in a sensuous presentation apprehended as conditioned by something other than itself."

[2] The term "imaging," therefore, is one to which a very wide extension is here attached. It may apply to all processes by which reference is made definitely to an object or to parts of an object in the absence of direct sense experience to guide the reference. It includes all the cases in which visual, auditory, verbal or any other of the commonly recognised forms of images are present; it may perhaps include instances in which none of these can be discriminated; and it covers all the examples of 'imagining,' in which our attitude is definitely a calling up of something that is 'not here.' But, as is indicated later, it is distinguished from thinking in that the latter deals primarily with relations and not directly with specific instances.

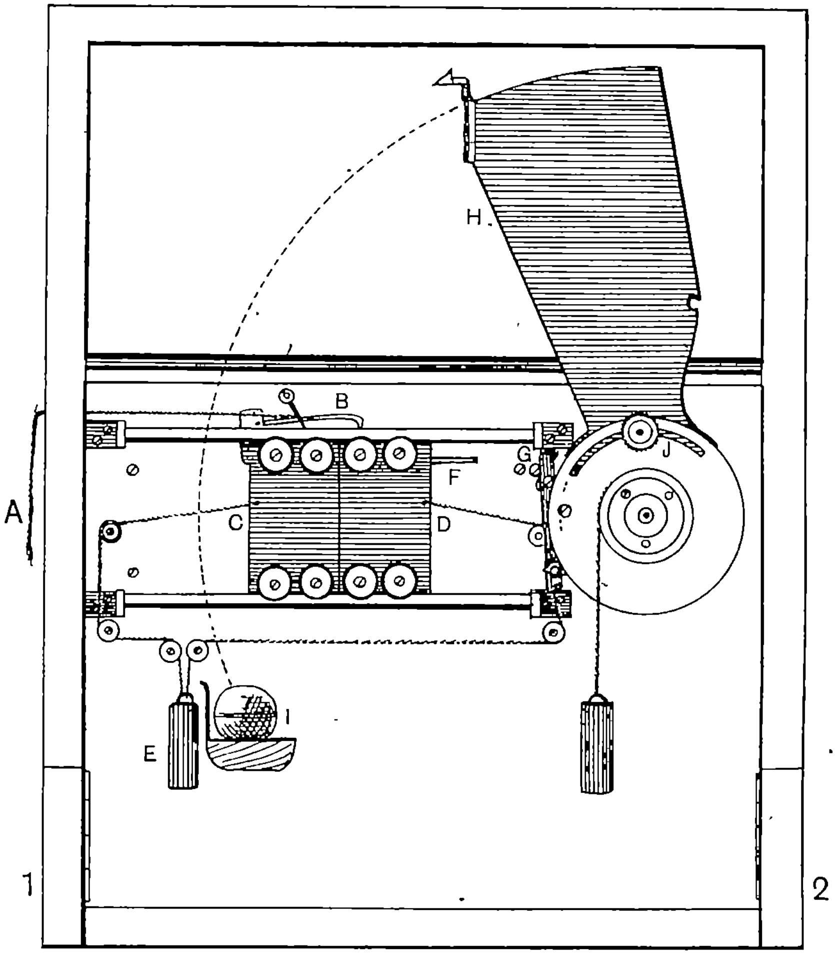

Fig. 1. The experimenter pulls the string *A*, and thus releases the catch *B*, which holds together the two shutters *C* and *D*. The shutters are pulled back by the weight *E*, and the rod *F*, on the shutter *D*, hits against the lever *G*, on a small projection from which rests the shutter *H*. The lever is pushed back slightly, and the shutter falls into position in front of the open window, its fall being broken by the air cushion *I*. *H* is held in position by the catch at the end of the shutter.

The tachistoscope is set up on an ordinary table, and rests on folding brackets at 1 and 2. The subject sits at the table on the other side of the shutters, with his eyes on a level with the window, and he sees the object—which in these experiments was placed upon a card rest set at the proper height—for the period between the running back of the shutters *C* and *D* and the falling of the shutter *H*. The distance of *H* from the window can be adjusted by means of the thumb screw at *J*. The parts of the card rest can be screwed to the body of the instrument, and the whole folded so as to take very little space.

material by means of the Hales tachistoscope[1], and in the case of the designs and some drawings by means of a small portable tachistoscope which was specially made for use with these experiments, and which did satisfactory work throughout (see Fig. 1). The advantages of the smaller instrument were that it could be carried about and set up almost anywhere; it could be employed in daylight or with ordinary conditions of illumination; material could be introduced easily and as easily modified, while the fact that the shutters opened from the middle certainly seemed to give the composition of presented material a better chance of producing its ordinary results.

With this instrument the commonest length of exposure used was about ¼ sec. In justification of the shortness of this it must be remembered that the ordinary glance of everyday life does not rest long on any given object, and also that the attitude of the subject in experiments in which he is definitely set to observe and to reproduce is far more keen and critical than it is ordinarily. A short exposure may therefore help to get near to everyday conditions. When the less simple figures were used, however, subjects often called for repeated inspection, and this was allowed. Repeated exhibition was resorted to also when it was clear that a good deal of a presented figure was yet to be made out. This, as a rule, began to be the case when the designs in *Group* 3 of *Series I* were reached. In all such cases the subject attempted a reproduction after each exposure.

In the case of the simpler designs the subjects reproduced what they had seen by drawing it immediately after the observation. Often the drawing was supplemented by description, and whenever difficulty of draughtsmanship was experienced subjects merely described what they had seen. Few suggestions were made to the subjects, so that practically all the extra information given by them was volunteered, and must have consisted of features that seemed specially striking.

The material used falls naturally into series, and in what follows it is arranged roughly in ascending order of structural complexity. What appears complex to a subject however is not by any means necessarily that which contains the greatest amount of detail, and this order of structural complexity is adopted simply because it is convenient for purposes of arrangement.

A different method was employed for the definite study of acts of imaging. That however will be described later.

In connexion with the first four series thirty subjects in all were tried, though they were not all presented with the whole set of figures. All but one of these were adults, the one exception being a youth of fifteen. Six of the subjects were women. The persons who submitted to trial had very varied interests; six of them were from a small country town and their interests were chiefly practical; twenty-two were graduate or undergraduate members of the University of Cambridge, and the other two were working-class men from a fairly large town.

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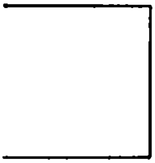
[1] See this *Journal*, II. 244.

For the tests on imaging thirty-six subjects were tried. Only four of these were children, and the rest were adults. They were diverse in interests as in the first group of tests.

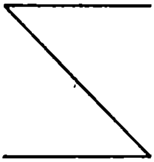
3. Description of Experiments and of Results.

(i) *Series* 1 consisted of fairly simple designs and patterns. The figures used fall naturally into three groups, and in what follows a selection is given from the figures of each group.

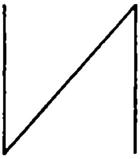
(*a*) *Group* 1:



(i)



(ii)



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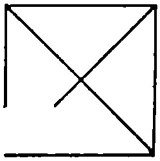


(iv)

With such simple figures as these the attitude of all of the subjects was practically the same. The designs were seen as wholes and were reproduced without hesitation. There was no attempt to analyse into parts.

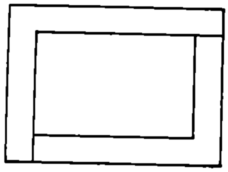
Names were given very commonly indeed except in the case of (i), and even this was often called "a square with one of its sides gone." (ii) was said to be "Z upside down" (though it is not this of course); (iii) was called 'N'; and (iv) was a "square with diagonals." The names were generally given as soon as the design was exhibited, and they appeared to induce in the subjects an attitude of greater certainty and satisfaction. The tendency to give names persisted throughout the whole of the experiments, though it was much more marked in some subjects than in others. For diagrammatic material it was least used by those subjects who had been much occupied with mathematical study.

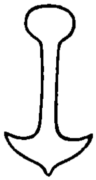
With quite simple figures there was no reliance on the name as a guide to representation. Though (iii) was called 'N,' it was correctly given by all subjects, and (ii) also was generally reproduced accurately. In all these cases the naming seemed to do no more than make the experience definite.

The gaps in (iv) caused difficulty. Whenever a design was given in which a gap or any unusual feature was present, it was common for the unusual characteristics to be noticed and reproduced, but there was great difficulty about determining their position within the figure. This accords with common experience. We readily notice any unfamiliar feature in fairly familiar objects, or anything unmeaningful in figures that carry a common meaning. The familiar requires little attention. At a glance we interpret it. But a gap or an unusual line attracts and holds us. So we remember *that* it is, although we may forget *where* it is. The whereabouts, in fact, we can rarely give, because it is not the whereabouts that we attend to, but the gap or line itself. So most subjects were doubtful about the accuracy of their reproduction in these cases, and would say: "I know that this gap (or line) is there, but I am not sure where to put it." Partly, no doubt, this was due to the shortness of the exposure. But it is notable that the unusual should almost always be seen readily. Not all changes, however, appear to be equally noticeable, as results gained from the kinematographic material (Series 2) used later tend to show.

Even with the simplest figures interpretation tended to run beyond what was presented. Especially when a gap or line arrested attention there was a marked tendency to suppose that the rest of the figure was complete and of some common shape. Most subjects, for instance, gave the square of (iv) completely, but reproduced correctly the gap in the diagonals. The tendency to interpret what is not seen in the light of what is seen was more marked the greater the detail of presented material and the greater its familiarity.

(*b*) *Group* 2:


(v)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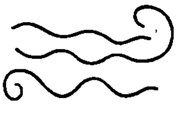
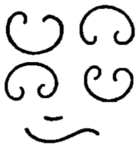

(vii)


(viii)

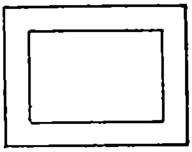
The designs in this group contained slightly more detail than those in Group 1. Some of them, of which (v) and (vi) are illustrations, were such as to suggest more readily representations of concrete material, while others—(vii) and (viii) are instances—were more meaningless, and were composed of more disconnected parts.

The attitude adopted in these cases hardly differed from that taken up towards the designs in Group 1. Always the attempt was to take in the figures as a whole, and to reproduce all of a given design after a single glance. Most subjects now hesitated a little longer before they attempted to put down what they had seen, and this tendency to pause increased with practice. In every case it was said to be due to the fact that in this way it was most easy to get a clear image of the object seen.

Naming now became of greater importance than before, in that it began to play a definite part in shaping representations. For example, (vi) once recalled a 'pick-axe,' and was represented with pointed prongs. Once it was called a 'turf-cutter,' and the blade was made very much rounded. It was said to be a mixture of 'a key—the handle—and a shovel—the blade.' Another subject remarked that he was reminded of a sign he had once seen at a railway station: "It was a picture of a spade, of two spades in fact, only they were not that way up. But this is more of an anchor." Five others also called the figure an anchor, and in these cases there was some tendency to exaggerate the size of the ring at the top. Curiously enough, only once, when the figure was called an anchor, was the absence of a cross-bar at the top commented on. It is possible however that the exaggeration of the size of the ring was due to a vague apprehension that there was something unusual about the top part of the figure. That such vague apprehension is fairly common was shown clearly enough in connexion with figures used in Series 2.

Once only was the point in the blade of (vi) correctly reproduced, and on that occasion the subject said that the design represented 'a prehistoric battle-axe.'

Exactly the same function of naming came out in connexion with (v). Twice it was called a 'picture-frame,' and the reproduction given in these cases was



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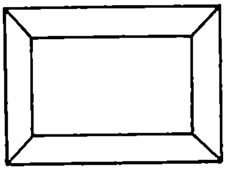

respectively. But the subject who spoke of it as two "carpenter's squares placed together" got the figure right at once. Another subject was reminded of a garden plot with paths round it. This of course did not help at all in the reproduction: "I am not quite sure," he said, "how the paths go." Indeed in this case it was the association rather than the figure that attracted his attention: "I was reminded of the sums I used to do; so many tons of gravel at so much a stone."

All this illustrates also how great a variety of names are likely to be given even to very simple figures[1]. The variety arises at least partly from the fact that what led to the name was usually a general impression, together with a definite observation of certain detail. The general impression suggested the nature of the whole given, and the detail was used to help in the interpretation of the whole. The direction of the interpretation was largely determined by some earlier experience of the subject. In that way features not present at all often came in, and present features were readily modified.

Goldscheider and Müller, in their account of experiments on the process of perceiving in reading, speak of memory as simply a way of functioning of the activity of apperception. Whatever the value of such terminology may be, it is certain that even with very simple figures, the process of perceiving was, in these experiments, shortened, and at the same time laid far more open to error, by the tendency to interpret presented material in accordance with the general character of earlier experience.

This itself however is just a way in which what seems to be more fundamental still finds expression. Always in perceiving there is present what may be called an *effort after meaning*. The exact significance of this will have to be discussed later on. Whenever material is presented there is a tendency at once to seize upon and to use any factor that will help to free the act of apprehension itself from an accompaniment of felt strain. Thus with familiar material there is immediate reference to the general nature of past experience, while if the material is unfamiliar other factors may be used to bring about the same result.

This leads to a consideration of the results arrived at with designs of which (vii) and (viii) are taken as illustrations. Both of these are relatively meaningless, and both contain disconnected parts. They gave more trouble than the other figures in the group, and subjects were generally dissatisfied with the reproductions they were able to achieve.

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[1] Cp. Quantz: "Problems in the Psychology of Reading," *Psych. Review*, Mon. Suppl. II. No. 1, p. 10. "◇ is called a diamond or a rhombus; ○ a circle or a globe; ☾ a moon or a crescent. This is returning to the indefiniteness of picture writing."

The difficulty was greatest with (vii), for there is an order of arrangement in (viii), and anything symmetrical is very quickly noticed by most subjects. That which is so much outside ordinary experience that it is hard to find for it a name, and which at the same time presents an apparently arbitrary mixture of lines and curves, most effectively resists the 'effort after meaning.'

Often however when figures in themselves appeared relatively meaningless they were given a sort of significance by analogy with other figures. And here the naming became, if anything, more important still. For the analogy had practically always to do with the shape of a figure, or with the disposition of its lines and curves. When (viii) was exhibited to a mathematical student, for example, he remarked at once: "That arrangement of lines reminds me of what is called a 'determinant.'" His reproduction was accurate after a single glance, and several weeks later he still remembered and reproduced accurately the shape of this figure. Analogies of this kind were frequently used. The fact that when interpretation is hindered in one direction it will work out in another helps to justify the use of the term '*effort* after meaning.'

In spite of the difficulty of interpretation, the common attitude in perceiving remained the same with these figures as with those formerly used. Even when the figure was exhibited three or four times, subjects practically invariably tried to make out the whole at one glance. It would seem in fact as if there is a more or less conscious standard in such cases. When the structural complexity is not very great the attitude is: "I ought to be able to make out the whole at one glance." Accordingly the effort is made, and it is only after failure repeated that a piecemeal method is adopted.

Another point, many times illustrated later, was first noticed with (vii): the tendency to a multiplication of detail with disconnected material. In some subjects this tendency was very strongly marked[1]. They were instances of the persons who not only almost always try to take in everything at a single glance, but who are readily confident that they can give a fairly good reproduction. "In every case," said one, "I kept my gaze fixed on the screen for a few seconds after the window had shut down. I was trying to get a clear image of what I had seen, and in doing this I usually felt part of the design escaping me, while the rest set into a firm enough shape to be transferred to paper. The sense of having lost some part of the original may account for the general superfluity of the reproductions—a desire to make up for the bareness of the actual image by adding *probable* lines here and there."

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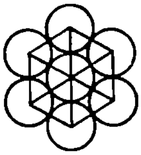
[1] In discussion of this paper Dr P. B. Ballard made the interesting statement that he has found this tendency to multiply details common, constant, and very marked among children. This he found to be particularly the case among young children who were unable to count. Inability to count was of course the lot of all subjects in the experiments that are described in this paper; the shortness of exposure made definite counting generally impossible.

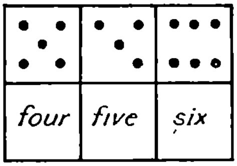
Apart from this, there is, no doubt, in many cases, an immediate impression of crowded detail. The attempt is to take in at a single glance all of the features of whatever is given. But when these are more numerous than are readily apprehended or remembered they can be given only very hurried attention. The hurry itself suggests that there is more to be seen than is really given[1], a general attitude is induced in which the subject says hopelessly: "I shall never get all that," and this again helps to heighten the impression of detail. The confident subject falls readily in with this attitude and gets down in his reproduction more than is to be seen; the cautious, hesitating subject reacts in the opposite way, and tends to diminish rather than to increase the details presented.

The importance of such general attitudes in determining the result of an act of perceiving will be further illustrated later on. They also are evidence of the way in which factors which are to be traced to the past life of an observer may affect the results of experimentation.

(*c*) *Group* 3:



(ix)



(x)

The designs in this group still formed readily appreciable wholes, but they contained rather more detail than the figures used hitherto. Some of them, of which (ix) is taken as an example, were such that it seemed likely that they would be best reproduced by subjects who grasped the plan of their construction. Others (*e.g.* (x)) were devised in order to see whether, with material divided into parts,—the parts themselves containing considerable or important detail—there would be any tendency to a definite change of method on the part of the percipient.

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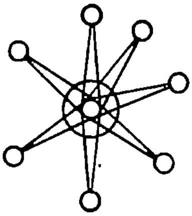
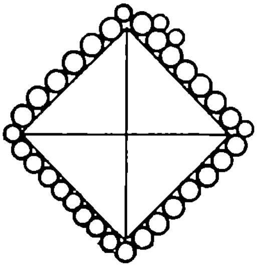
[1] Cp. Emerson on *Prudence*: "The Latin proverb says 'in battles the eye is first overcome.' The eye is daunted, and greatly exaggerates the perils of the hour."

Design (ix) was called simple by some subjects and difficult by others. All those who called it simple were familiar with mathematical constructions; all those to whom it was difficult were unused to dealing with such material. Here came prominently into notice the factor of the general attitude involved. When a design was exhibited a subject's attitude would be either an easy and contented: "Yes, I've got that," or a puzzled: "I shall never be able to get all that." Often, indeed, the first attitude would turn into the second, and many subjects remarked that they were sure of the details of designs seen until they began to try actual reproduction, but apart from such changes the general attitude of ease or of satisfaction certainly affected the nature of the reproduction.

Here for instance are two attempts to reproduce (ix), both done in the attitude of "I shall never be able to get all this":





It was not pretended by the subjects that these[1] were accurate reproductions, but they were proposed as "something like what I have

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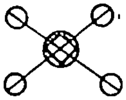
[1] The first of these was very probably partly due to confusion of (ix) with some of the characteristics of the figure given immediately before. This was:



Professor Carveth Read suggests that considerations of this sort show that the experiments included much more than perceiving proper. The subjects did not *see* exactly what they reproduced. This is undoubtedly true, but it seems simply to reinforce the position that a very great deal of what we call perceiving in the ordinary way is enormously influenced by processes or acts of imaging, remembering and the like.

seen." It will be noticed that in both cases, and particularly in the second, the number of small circles has been considerably increased. Yet in the second case the subject said that his reproduction was faulty because "there ought to be more circles in it."

On the other hand, to two subjects who were well acquainted with geometrical construction the figure presented no difficulties. They got the key to it at once, and reproduced it practically correctly at the first glance. What they said of it was "I know how it is made," and "I have been accustomed to that sort of figure for a long time. I know for instance that there must be six small circles round the middle one." In fact the general attitude of satisfaction with which they faced this design was practically the same as that with which most subjects met the concrete picture material of later series.

The change from satisfaction before reproduction was attempted to dissatisfaction when the attempt was made was no doubt partly due to the common swift fading of the primary memory image. But it was probably more definitely due to the fact that in ordinary circumstances there is very little need for accurate, detailed reproduction of what is observed. Ordinarily general impressions, as we call them, are far more useful to us than very detailed observation would be. Accordingly we may be perfectly well satisfied that we have quite well grasped the nature of presented material, though we may all the while have no clear apprehension of detail at all.

With (x) came a very noticeable difference in the general method of perceiving. The single glance method having served tolerably well up to this point, it was always tried first when (x) appeared. But invariably it resulted in failure. Then, on repeated exhibition, quickly in most cases, more slowly in others, a definitely piecemeal method of observation began to be adopted.

The general plan of the figure was commonly obtained at the first, though sometimes eight squares were given instead of six. There was never at first any certainty about the contents of the squares.

Then the small squares were taken one or two at a time till the whole figure was covered. Commonly, with this method of concentration, unless a part of the figure was definitely attended to, it was barely seen at all. Thus errors made concerning any particular square were left unaltered, even after repeated trial. If he was shown the design afterwards, and the error was pointed out, the subject would say: "I thought I had that right, and afterwards I did not look at it at all." Partly this was due to the shortness of the exposure, but it also fits in with the already noted tendency to rely for details on a general and ill-defined impression.

It seemed likely that there would be an effort to connect the words in the lower line of squares with the numbers of the dots above them. But as it turned out there was little or no tendency to do this. The common practice of taking each square by itself accounts for this result; and when the figure was seen as a whole there was very little discrimination of detail.

But one point was very clear, and that was the tendency to take the top part of the figure first. In 70 % of the cases attention was given first of all to the top line of squares. The same was found over and over again with other figures in which the detail was rather too great to be clearly apprehended all at once. And particularly in the next series, when symmetrical material, arranged so that the bottom part simply repeated the top, was employed, the report constantly given was: "I saw the top only," or: "It was the top part I saw clearly." All this was a fresh illustration of the fact that, as Huey points out[1]: "we habitually find most meanings in the upper parts of objects; we ourselves are so placed and so oriented as to bring this about."

(ii) *Series* 2, *Kinematographic material.*

We pass now to a consideration of Series 2. So far there had been no definite structural relation between the various figures used, except that they were roughly arranged in an ascending order of complexity of detail. But now a series of designs was employed in each set of which there was a gradual increase or decrease of detail, the plan of construction remaining the same throughout. By the use of this sort of material it was hoped to make a little more clear some of the conditions under which definite changes in presented material are most likely to be observed, and to find what sort of changes are observed most readily. A single illustration will suffice to show the kind of design that was used. (See following page.)

The designs were presented either in ascending or in descending order of structural complexity. They were not all quite as regular in their changes as those given above; in some lines were dropped as well as added, in others symmetry was lacking, and in others the first or else the final figure was a representation of a common object, such as a bird or a crown.

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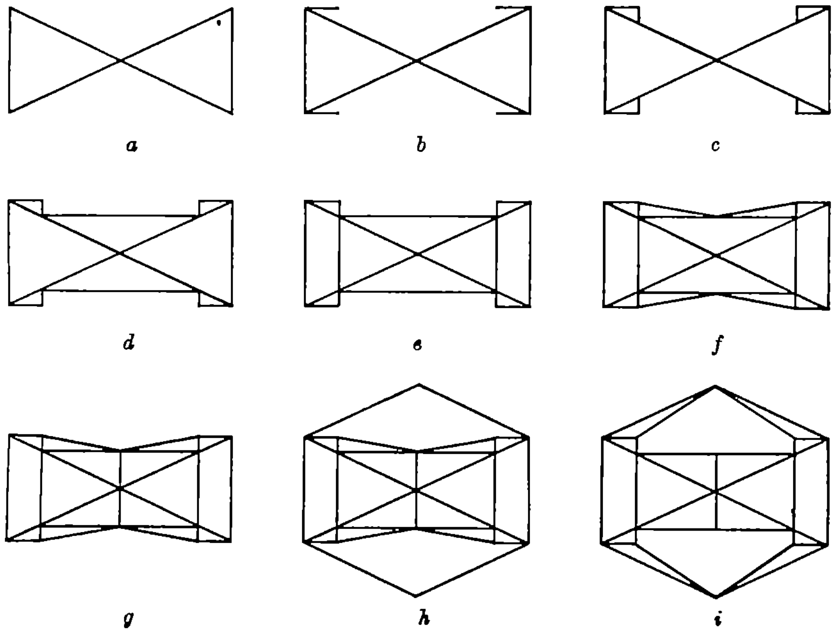
[1] Huey: *op. cit.* p. 99.

The tendency on the part of all subjects to notice particularly the top parts of the figures, unless some special reason attracted them to another part, has already been mentioned, and no more need now be said about it.

Symmetry was noticed at once by all the adult subjects. "I got an impression," they would say, "that the different parts of the figure were alike." Symmetry certainly plays a large part in determining that composition of an object which is most easily and accurately perceivable. But the really interesting point was the use in this connexion of the very common phrase "to have an impression of." This, and the alternative phrase: "to have a feeling of" were often used of the perceiving of symmetry, of similarity, of sameness, of difference, of the 'progressiveness' of a series of figures, and occasionally of 'meaning' in the sense of some representation or other. And this 'feeling,' as subjects called it, was commonly made the basis of important inferences.



This was best illustrated in cases where a figure became progressively more complex, while its symmetrical nature was retained. "I got an impression," said one subject, "that the figure was symmetrical, though I did not notice the details. I built on that, looking for one addition or omission, and then inferring others in other parts of the figure." Another said: "I had an unconscious assumption"—he apparently meant precisely what others meant by 'impression' or 'feeling'—"that the figure was progressive, and relied upon that." And another: "I looked for some alteration as the different figures came. I had a feeling that the figure was growing more complex, and so I looked to see if anything had come in, and not if anything had gone out. I certainly did not see the whole of the figure in its later stages, and unless I had seen it in its simpler forms, I could not then have reproduced it at all." Thus there was a general impression of the composition of the figure, together with a definite noticing of certain parts, and an inference based on both resulting in a representation of the whole. Unless the plan of construction had already been firmly grasped, strict concentration upon detail was a downright hindrance.

Further evidence of the importance of the inferential element was afforded by the difficulty found with curves. "When you have a straight line," said one subject, "you know where it *must* go, and if you have an impression that the figure is symmetrical and notice two or three straight lines, you can join them up, and make something of what you have seen. But curves might go anywhere. You can never tell."

Even slight differences between figures, the mere omission or addition of a line, were readily noticed. This, as many subjects suggested, was due in part to the general similarity of successive designs. Subjects, having got the general ground plan of a figure, would assume that that remained constant, and look specially for differences, often of a particular kind[1]. But not all differences were equally easy to notice. Of the fourteen subjects who tried this series, eleven considered that additions were easier to notice than omissions, one thought that they were equally difficult, and the remaining two "did not notice." It is not altogether easy to decide, because when the plan of progressive additions is adopted the figure is less complex to start with, and all its parts are easily discriminated. But on the whole the evidence of the reproductions shows that additions are more commonly noticed than omissions, and particularly are more likely to be definitely attended to, and their position accurately reproduced. When something is dropped out, the result is often that vague experience called by most subjects an 'impression' or 'feeling' of difference. Thus after having worked through the set of figures given above a subject remarked: "The base of the whole set was something like a box. When I got down my representations I could see I had left parts out. I had a vague idea of what was omitted. Each time I had a general impression of the whole figure, but some details always escaped me before I got them down. On successive exhibitions I noticed differences without being able to say just what was different." That experience was common, but the indefiniteness of it was more marked with omissions than with additions.

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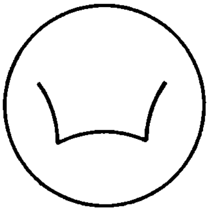
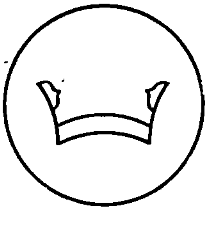
[1] Cp. Royce: *Outlines of Psychology*, p. 92, "In the effects of decorative art the similarities present, for instance the symmetries, help me to appreciate more definitely the differences of experience upon which the decorative effect depends."

Obviously, further experiments ought to be made to determine the readiness with which other changes are noticed. Never once, in any of these cases, did a subject fail to note, when a diagram was turned round or put upside down, that it was the same figure in another position.

It was noticeable how readily expectation arose upon the repetition of the same general situation. Two or three exhibitions of diagrams in which lines were added, and the subjects would definitely begin to look for additions. If something quite different from what had been expected turned up, the reproduction would suffer. Again, expectation arose readily whenever figures carrying a familiar meaning were presented. One of the sets was a drawing of a crown, the first figure of the series being as follows:



With the exhibition of the second figure:



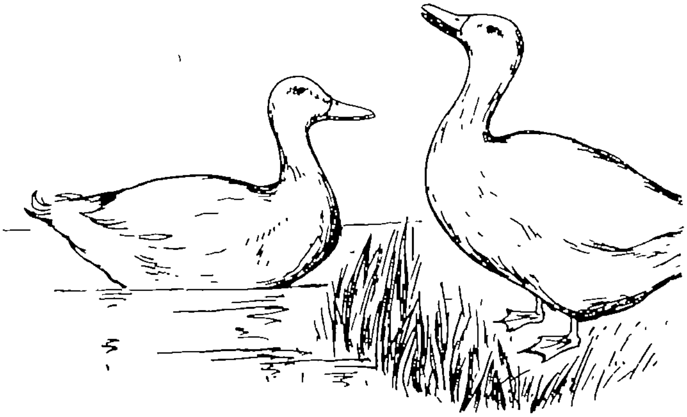
subjects began to guess what the final result would be, and at once to expect the nature of coming changes. The expectation did not work out into any close detail, but it did concern definite parts of the figure. "I was expecting something in at the top," a subject would say. Here again was evidence of the part inference may play in determining the general attitude and result in an act of perceiving. What was expected was a result of inference from what had been presented, and from the nature of earlier experience.

It was interesting also to notice how a complete figure was apt to appear more simple in construction than the same figure in an incomplete state. Over and over again the final crown was judged to be a less complicated figure than others which preceded it. As one subject put it: "The unity of the figure made it appear more simple." This sort of completeness of course is most likely to be carried by definite representations. It seemed to be based on the readiness of recognition which was thus made possible, a readiness which did not depend on the clearness with which details were discriminated. A subject who began with the most complicated figure thus remarked at once: "That is a crown," adding, "But I have not got nearly all the detail. I am going to draw my idea of a crown—a conventional crown—rather than exactly what was given." For all ordinary purposes in perceiving an object of this sort we never bother much about the detail, but perceive in accordance with a general idea which we already possess. In these experiments, such general ideas played the same function in reference to common objects for all subjects as did an appreciation of the plan of construction of a geometrical figure for the mathematical person.

(iii) *Series 3, Simple concrete representations.*

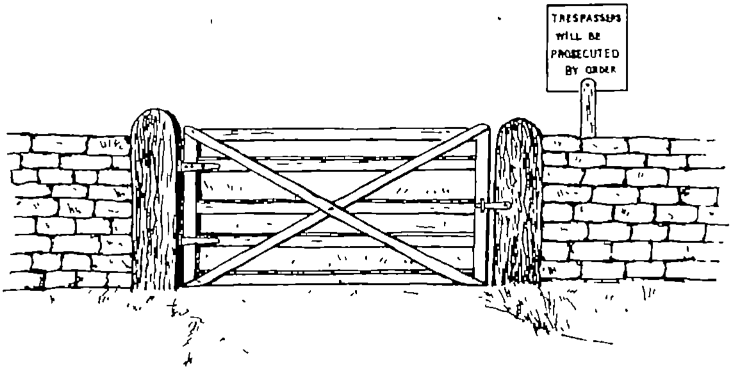
This leads on to a consideration of Series 3. Many subjects complained that diagrammatic material was uninteresting and to them unduly difficult. They were, they said, unused to dealing with such material. They suggested that they would get results more satisfactory to themselves if they were given representations of familiar objects. So another series was used, consisting of line drawings of common things or scenes. The following three reproductions will suffice to indicate the kind of material used.

This picture material was received generally with a very marked difference of attitude. Most subjects welcomed it with great relief. "I feel more satisfied that I have made out a picture of this kind," said one, "than that I have made out a design. I am no good at lines



(i)

[Reproduced by kind permission of Messrs Harrap, English agents of Macmillan and Co., from Huey's *The Psychology and Pedagogy of Reading*.]



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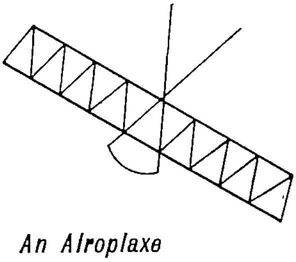

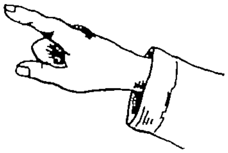

(iii)

and circles because I am not used to them." And another: "Yes; that is better, I can easily get the whole idea here and I need not bother about the details." Twice, however, a preference for the designs was expressed, and in both cases the subjects were mathematical students who were familiar with diagrams. Their reason for liking the figures was of the same nature as that of the ordinary person for preferring the pictures. They grasped readily the plan of construction of a figure, while the ordinary person at once appreciated the general meaning of a picture. In neither case was strained attention to details called for, and that is just the position in most everyday acts of perceiving.

It is again clear from this that the figures which appear simple are by no means necessarily those which contain the fewest number of strokes, but rather those whose significance is the most readily interpreted. For the ordinary subject, who has no specialised interests, that means that the simplest material is either actual common objects or scenes, or representations of these. For the persons with specialised interests the simplest material is somewhere within the range of their particular bent; hence matter that appears to one person of enormous complexity may seem simple to another. And as far as perceiving goes, the simpler the object in this sense, the less likely is there to be any definite attention to particular details presented, and the more likely is the subject to rely largely on earlier experience for his interpretation of what is given.

With straightforward representations such as that of the duck scene above there was little difficulty. The general tendency was to see the picture as a whole at the first glance. The common description was very much like the following, which was given after a single observation:

"That is a couple of ducks. One has its head to the left and the other to the right. One is standing up and one is in a pond. There are reeds growing where the one is standing. They are rather big for ducks."

It is interesting that simple critical remarks, such as that of the last sentence, commonly came in at once. Drawings were called 'bad,' or 'rather effective,' or 'like a child's'; objects represented were commented on, and the critical comments often enough preceded the first description. The valuing attitude was in fact very closely connected with the act of perceiving itself. The tendency on the part of adult subjects to say something *about* material presented, as well as to say *what* is presented, was coming out all the way through. It was by no means confined to people well versed in contradictoriness, as philosophers or psychologists, but was just as much marked in people with practical and open-air interests who submitted to trial. In fact the valuing may quite well be called a part of the total act of perceiving, whether it takes the form of an unspecified satisfaction or dissatisfaction, whether it stops short at a mere "I like (or dislike) that," or whether it proceeds to definite criticism of details. Just as for all of us in practically all cases there is a general rule or plan helping to determine how and what we perceive in a given case, so also there grows up a tendency to accept, or reject, or anyhow in some way to criticise, whatever is presented, and this also plays its part in determining the result of an act of perceiving. By adult subjects the valuation, when it is applied at all to details, is generally carried out through a kind of comparison of what is presented with a rather vaguely recalled standard or conventional representation, as in the duck case above. The comparison is not, of course, definitely recognised as such by the subject. It has its origin on the feeling side of perceiving, though it may seem to get entirely free from that with the growth of experience.

The other two examples here given help to bring out the considerable influence of already acquired knowledge, concerning the nature of the general situation presented, in determining the result in perceiving.

In (ii) the point of interest was the notice-board. This was generally seen at once, but the writing on it could not be distinguished. With repeated exhibition subjects concentrated on the writing, but in no case did they succeed in reading the words. Practically always however suggestions were volunteered.

Eight subjects out of ten guessed that the board contained "Trespassers will be Prosecuted." One said: "I seemed to see it vividly. It is foolish, I know, because I can't read the writing, but I seemed to see 'By Order' written underneath." Once, influenced, he thought, by the closed gate, a subject gave "No Road" as the sign, and another gave "Keep off the grass." The last subject at once added: "I wonder why I gave that. It doesn't look as if it would be that, of course; it is much more likely to be 'Trespassers will be Prosecuted.'" The reason commonly given for this choice was that most boards seen in such a position are found to contain that inscription.

In (iii) the details were purposely left disconnected and the representation made far from definite. In all cases but two however the suggestion of an aeroplane came at the first or second glance. Usually a subject was at first not quite sure. The relative unfamiliarity of the main figure attracted his chief attention to that, but there was almost always, at the same time, an impression of something undetermined on the left and right beneath. Later attention was turned to these features. In every case but one the writing was said to be 'An Aeroplane.' The misspelling was not noticed. Once a subject made out the writing before he named the main object, but in all other cases the reverse order was followed. In one instance only was the word read correctly, and this was the case of a man who had already seen the figure several times without being able to make out what it was. He, that is, turned to read the words with no idea whatever concerning what sort of object was represented.

One subject only failed to see the hand as a hand. His failure was interesting, for it further illustrates the importance of a general topic or interest for the act of perceiving. He said: "To the bottom, on the right, is what I take to be an upraised cannon pointing at the aeroplane." The experiment was carried out, it should be noted, at a time when German air-raids were common, and one upon the town in which this subject was living was quite within the bounds of possibility. Moreover this subject was primarily interested in engineering.

What was seen therefore of material in this series depended largely, as in the other cases, upon a general impression, gained from a first glance, of the plan or topic of the object given. But while in the diagrams the main factors were such as those of symmetry, the greater definiteness of straight lines as opposed to curves, the likeness of figures presented to familiar geometrical designs, and here and there the representational value of the figures, with concrete picture material the main determining factor throughout was the meaning carried by the objects presented as representations, and the meaning itself was often fixed by the subjects' constant or transitory interests.

One noticeable feature of all these series ought here to be mentioned. In many cases when repeated inspection was called for, the opportunity was taken to substitute for the original figure one in which some detail was added, or omitted, or into which some other change was introduced. The details of results must be left for more comprehensive treatment, but one common fact must be noticed. Subjects were able to appreciate very slight differences, but even when the differences were most clear, there was a strong tendency to refer them to original faulty observation, and not to change in the object itself. For instance seven dots were arranged in a certain order on two cards. On one card all of the dots were red and on the other three were black. When the second was substituted for the first, every subject noticed the difference, but not one at first attributed it to any change in the card itself. They were surprised at the change, nevertheless they at once thought that their original observation had been wrong. It was only when the alternation had been two or three times repeated that they judged that two cards were being used.

This strong tendency to refer change in sensible appearance to faulty observation seems to be largely due to just those factors which lead also to great variety of interpretation of the same material.

(iv) *Series* 4, *Complex picture material.*

So far all the pictures used were simple in construction, and contained relatively little detail. In experiments carried out with a somewhat similar purpose by Mr Frank Smith (see This *Journal*, VI. pp. 320–362) concrete picture material rather detailed in nature was exposed by means of the Hales tachistoscope. It seemed desirable to complete the passage from structurally simple designs to really complex representations, and accordingly pictures containing much more detail than those used in Series 3 were employed, and these constitute Series 4. For most of them the pendulum tachistoscope was used as a means of exhibition.

There are, however, objections both to the use of this sort of material and to the method of exhibition, and it is rather as forming a very good introduction to the definite study of imaging that the results of this series are here worth consideration.

When the pendulum tachistoscope was used it was often found difficult to get a clear definition of the objects represented, except with a rather small picture. Some subjects also are troubled by having to stay for a length of time in a dark room. In any case the method of presentation does not put a subject into anything like a normal situation. We do not commonly have to make out complex pictures or scenes by a series of flashes[1]. We may, of course, often enough observe, by a number of rapid glances, a landscape through which we are passing. But in that case we are not anxious for accurate reproduction, and we rarely need to observe closely. It does not seem safe to say that the flashing up and out of a scene can give a result at all comparable to the continued observation of that scene, even when that observation passes from detail to detail. Thus the problem set by these experiments hardly ever approximates to any problem set in the course of ordinary observation. The methods adopted by the various subjects may therefore be passed over rapidly, seeing that probably they are largely determined by the peculiar conditions of the experiment.

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[1] It is true that momentary exposures were used throughout, and that to some extent this method is bound to tend to produce an unusual attitude in the observer. But the difficulty arising in this way is less marked the less complex in structure is the material given to be observed.

The main points of interest are:

(1) In all cases the first glance or two was used to gain the general meaning of the picture. The earliest description generally contained a good deal of detail, but the subject was guarded and doubtful about this. With unfamiliar scenes attention was turned upon detail more quickly and definitely than in the cases in which the presented material was familiar. The composition of the picture was important; a central figure quickly attracted attention and other details were grouped around this.

(2) Later observation went definitely to detail. Some subjects were more systematic than others in the way in which they explored what was given. These however did not get any more accurate results than the rest. Commonly details were interpreted in the light of the idea of the whole gained at first. In detailed observation subjects generally asserted that only that part of the picture was seen to which attention was specially directed.

(3) There was generally a period of increasing dissatisfaction with interpretations. Certain details were seen to have been observed wrongly, and doubt spread over the whole. Generally this passed with repeated trial, though at times it remained to the end.

(4) Associations of various kinds were very common, and usually increased in the later stages of exhibition of a particular picture. For then, as subjects said, they had: "seen the thing so often that there is nothing much else to think of."

But it is more interesting to study the extraordinary differences of interpretation placed by different subjects on the same material. Variety in interpretation tended to increase throughout with increase in the amount of detail presented. The greater the detail the greater is the tendency to pass from what is seen actually to a construction of what is seen only imperfectly, or is not noticed at all. Imaging comes in more and more. The pictures now used were all rather full of detail; the subjects knew that they were expected to make something of them, for they represented concrete scenes, while the fact that the experiments took place in a dark room meant that there was practically nothing beside the presented picture to attract the subject's attention.

All this may have tended to increase greatly the part played by acts of imaging.

The most striking illustrations of this were given in the different interpretations placed upon a representation of the well-known painting of "Hubert and Arthur," by W. F. Yeames. Every person who was given this picture to describe made of it something different from everybody else. A few cases may be given. Repeated observation was always found necessary.

At the first glance subject A. said:

"It is a woman in a white apron with a child standing by her knee. She is sitting down and has her legs crossed. She is on the right of the picture as I see it, and the child is looking at her."

At the second attempt he said that the woman was standing up, and then, during thirteen trials, made few alterations and added very little detail. At trial 16 he said: "I had a vague feeling that I've seen it all before somewhere. But I don't know where, and I'm not sure what it is." At the next attempt he spoke of a "girl" leaning forward, and "stretching upward towards her mother—well, towards the woman." Further details were given, and then, at trial 25 he remarked: "Now I can see. The picture is that of a little girl saying her prayers on the other side of her mother's knee away from me. I mean she is kneeling on her mother's knee. She is dressed in a night-gown. The length of the night-gown made it look as if she is standing."

The picture was given thirty-eight times in all, but there was no further change in the general idea of the interpretation, though further details were given. The subject said that he had seen the picture in somebody's bedroom a long time before.

Subject B. at first saw simply two figures, but at the third attempt he said: "Yes, there are two figures. One of them seems to be leaning back a little, and the other is struggling with him, or is about to struggle." Thereafter the story was one of development of the idea of two persons wrestling. A "dark fellow" was made out, and was said to be "getting the worst of it." This subject saw the same picture fifty-five times.

Subject C. began in much the same way:

"I saw nothing definite, but merely a sort of contrast of black and white. There was something very like a white shape wrestling with a black one." At the second attempt he got his general setting. "Evidently it is a room with a black or shaded side to the right, and windows or else a highly illuminated part to the left. There was a black figure turning towards a white one. It was like a representation of Othello saying to Desdemona 'Come fly with me.'" There were alterations, and great additions of detail, but the subject stuck to this general idea throughout fifty-seven different observations.

Subject D. began very confidently:

"It is the interior of a house. There are three figures. One is tall, the second less tall, the third less tall still. The figures are leaning against a pillar or wall. Probably I am looking at a copy of some old master. It might very well be 'The Woman taken in Adultery.'"

But he soon withdrew this, and said that there were only two figures. He suggested that the picture was one of "Charles I and Henrietta," and to this he adhered through the rest of the trials, not however with any degree of confidence.

Repeated observation with varied material brought out the same thing over and over again. A simple silhouette picture of a man with a bundle of sticks on his back, leaning slightly forward towards a dog that is running just ahead of him, with a fence, a few trees, and a building in the background—the whole in white—was greatly changed by one subject.

"I seemed," he said, "to see a queen, and a man bending down over her. The queen seemed to have a crown on her head. There were boughs of a tree about her, and they seemed to be covered with frost." And then: "Yes, it is a prince bending down beside a queen. He has his head thrown back, and is looking up into her face. The dog must be a mere pet just walking along. It is cold." And once more: "She is dressed in costly garments. It seems to be one of the old-fashioned pictures. It reminds me of fairy tales and things like that; of adventure stories."

The last example shows how readily critical remarks (*e.g.* "it seems to be one of the old-fashioned pictures") and associations got mixed up with actual descriptions. Nobody else interpreted this card in quite the same way, but all such cases as the one given are certainly instances in which, to use the terminology of Avenarius, "Energie > Reizung."

Sometimes the picture exhibited called up representations of definite scenes. The best illustration of this occurred when one of the subjects, whose home was at a seaport town, was examining a picture of "Margate Lifeboat on the Slips." From the first he was troubled. At the 18th trial he said: "It is no use my going on. All the time I am getting a suggestion of the docks at home. And they are what I see, not the picture in front of me. One of the first things I did when I got a camera some time ago, was to take a view of that spot at home that

I was reminded of when I saw this. There was a ship of heavy freight there at the time, just as there is in the picture here. So I'm always confusing the two, and I shall get no more out of this."

This difference of interpretation, present to some extent throughout the whole of the investigation, but rising to its height so far in the series just described, is the point now of chief importance. It means that, in the interpretation of presented material, imaging, in the sense spoken of at the beginning of this paper, may play a prominent part. It therefore seemed worth while to attempt the study of a further series, which should be designed specially with a view to arousing acts of imaging; and for this to use outlines that might suggest many things, but that definitely represented nothing.

Before however the results secured from the use of this material are described, an attempt must be made to sum up the main points of interest that arise from the study of methods and factors in perceiving carried out with Series 1–4.

4. SUMMARY OF RESULTS FROM SERIES 1–4.

As to methods:

Methods adopted in perceiving varied with variation in the material given to be perceived. When presented material was comparatively simple in structure; when its parts were so clearly related by connecting lines that they formed readily appreciable wholes; when it was already familiar to the subject; or when it carried a common representational meaning, the general method was to attempt to make out the whole at a single glance. But with unfamiliar, or disconnected, or relatively detailed material the tendency was towards a more analytic method. The latter method was more readily adopted with complex diagrams of a geometrical character than with concrete picture material, except with those subjects who at once grasped the rule of construction of diagrams. The method of analysis was also more marked upon repeated exhibition of material. Then, commonly, parts only of presented material were definitely observed at any one glance, though most subjects had also what they called a 'general impression' of the rest. As in these experiments the purpose remained constant, nothing can be said with regard to the relation of dependence sometimes asserted to hold between change in method and change in purpose. In fact in these instances the change of method occurred most readily in those subjects who kept the constant aim clearly before themselves all the time, so as quickly to adapt themselves to any change in the material. The experiments show that a 'set' or habit of perceiving grows up very quickly indeed in adult subjects, and in the absence of definite decision.

Except in the simplest cases, the method adopted showed a close blend of perceiving and imaging. For reproductions often depended on inference from general impression and specific observations, and commonly contained detail which subjects admitted that they saw only imperfectly, and often something that was not present at all. 'General impression' may mean appreciation of a plan of construction—as, commonly, with designs—or of the subject of representation—as with concrete pictures. For ordinary subjects it is most important in the latter case. It greatly increases the rapidity possible in perceiving, but is unfavourable to accurate reproduction. Ordinarily however there is little need for very definite and accurate reproduction, and so reliance upon inference is often extremely important in everyday acts of perceiving.

As to factors influencing methods:

First there are those that may be referred to characteristics in presented material, and secondly those that may be referred to characteristics of the subjects themselves. There is close relation between the two.

Of the first class (*a*) with figures and diagrams:

(1) Symmetrical arrangement was found to be readily appreciable and favourable to correct reproduction. It was rarely observed in detail, but subjects would have an 'impression' of symmetry.

(2) With structurally complex material there was a tendency to observe the top parts of figures in most detail.

(3) Any novel features in presented material, particularly unusual gaps, were generally noticed, but their nature was more accurately reproduced than their position.

(4) Additions to a ground plan with which a subject was already familiar tended to be more readily reproduced than relatively equally great omissions from such plan. They were generally noticed *as* additions, and their precise nature was fairly well reproduced. With omissions, although difference was noted easily, the specific change was less readily given.

(*b*) With concrete picture material:

(1) The most important factor in helping towards successful perceiving was the arrangement of material in such a way as to form a readily appreciable whole of meaning.

(2) With detailed material composition was important. The most favourable arrangement was that in which there was a prominent central figure round which the other details could be grouped.

Of the second class:

(1) Most fundamental was the constant 'effort after meaning.' This accompanied and helped to direct all cases, but it is not to be identified with 'felt strain,' and does not necessarily imply the presence of such strain.

(2) There was a marked tendency to criticism or valuation, and this was definitely connected with the feeling that accompanied all acts of perceiving. In its simplest form it consisted solely in a general attitude of ease or of hesitation, but it readily expressed itself in the more definite forms "I do (or do not) like this," and in critical remarks about the designs or drawings given. The tendency to valuation was connected with the ease with which meaning was attached to figures, and with the nature of the meaning attached.

(3) What was made of the given material depended greatly upon the prior experience of a subject, and upon his constant interests or those of the moment.

(4) This reliance upon prior experience helped to produce great variety of interpretation, and may also be connected with the strong tendency to refer change in sensible appearance to faulty observation instead of to actual change in the object.

(5) It was common, particularly in subjects with no developed abstract interests, to give names either directly or by analogy to material presented. In many cases the naming helped to fashion the representation.

(6) When there were more details than a subject could readily make out at a single glance, and particularly when no plan of construction was grasped, there was a marked tendency, constant in any one subject, towards multiplication or diminution of detail. Closely connected with this were: (*a*) the attitude of dissatisfaction, (*b*) the swift fading of the primary image, and (*c*) the fact that ordinary observation is merely hindered if it concerns itself much with minute detail.

In several of these cases it is clear that factors that are of importance in determining the result of an act of perceiving may lie outside the limits of the act itself, and of these experiments.

(v) *Series 5, Ink-blots and ambiguous outlines.*

Series 5 consisted of ink-blots and ambiguous outlines.

The possibility of using ink-blots in connexion with a study of the imagination was suggested by Dearborn in *The Psychological Review*, IV. pp. 390–1. Material of this kind has been used by Dearborn himself in a brief "Study of Imagination" (*American Journ. of Psychol.* 1898, pp. 183–190); by Kirkpatrick in "Individual Tests of School Children" (*Psychological Review*, VII. pp. 271 ff.), and by Miss Sharpe in certain studies in "Individual Psychology" (*American Journ. of Psychol.* 1899, pp. 329–391). The material seemed to be very well adapted for bringing out certain of the factors in imaging, and therefore a series of blots was prepared, some of which are reproduced in the accompanying plates. The numbers here assigned to the blots do not correspond with the order in which they were presented.

Thirty-six blots were used in all. They were variously shaded or coloured, and were all on ordinary postcards. These were laid face downwards in front of the subjects. The instructions were:

"Here are a number of ink-blots. They represent nothing in particular, but might recall almost anything. See what you can make of them, as you sometimes find shapes for clouds or see faces in a fire." The subjects turned over the cards for themselves. The time of their reaction was recorded and was reckoned from the turning up of a card to the moment when the subject began to write down what was suggested.

Methods adopted may be described briefly. It has been noticed already how quickly a particular habit of carrying through a task set tends to arise, even in the absence of any defined plan. This found fresh illustration here. Nearly all of the subjects soon got into the habit of looking at the blots at arm's length, or of putting them farther off still. Several remarked, "It seems easier when I do this." The point is important because in the initiation of many ordinary processes of imaging clear cut definite details appear to play little part. It is in relation to that which *might* be many different things that images most readily arise, although at the same time the image itself tends to be of something in particular.

Suggestions made were commonly gained at the very first glance, though often some time was spent in elaboration. Times of reaction varied from less than a second to slightly more than a minute, but most cases of lengthy reaction were due to a subject's searching for the right word, or spending time in developing a vague suggestion already present. Sometimes it was the general shape of the blot, and sometimes it was an outstanding feature that played the chief part in determining the suggestion.

The most definite description of method was given by the subject who said that he 'rummaged about' amongst his images to find one that would fit a part of a given blot. He projected the image on to the blot[1]. If he got it to fit, and there were still parts of the blot uncovered, he tried other images as nearly as possible related to the first. In all cases the blot appeared to him as a whole. In this way he got often, as he said, scenes which he himself recognised as being quite absurd. Thus blot 13 on Plate II reminded him of "a lanky boy and a jester watching the antics of an inebriated abbot[2]."

Most subjects were far less definite, and a study of the results suggests that the most general case was that a blot presented immediately suggested a situation or a class of things, and that the rest of the work was further to specify this.

In the case of blots with several distinct parts there were well-marked constant differences between those subjects who saw them as a whole and those who took them merely as a number of different blots.

We turn now to a consideration of the nature of the suggestions made. First their general characteristics will be described, and afterwards an attempt will be made at classification on the basis of differences between groups of lists furnished.

The most immediately striking feature of the results was their enormous variety. Hamlet's swift changes of fancy about the cloud[3] were nothing to the varying verdicts of these subjects. What to one was a 'camel' (blot 2) to another was a 'tortoise'; to another a 'dog worrying a table-cloth'; to another 'two dead ducks and an ostrich'; to another an 'octopus'; to another 'a baby in a cot with a doll falling out'; to another a 'picture of Sohrab and Rustum in a book of Arnold's poems.' The uninitiated would hardly suspect that the following are all attempts to describe the same object:

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[1] The method of this subject could be more minutely described. First he *thought about* an object or a situation. Then, in his case in the visual form, he called up an image of some particular instance and this he tried to fit to the blot. The *thinking about*, which was quite common, is not to be regarded as itself necessarily involving an image in the ordinary sense.

[2] In general those subjects who took the blots as wholes got far more fantastic suggestions than those who split them into parts, and took each part by itself. Another person, for instance, found that blot 13 suggested: "In the middle the top of the apple tree; on the right the Devil with his horns; on the left Adam, and Eve in the distance."

[3] See *Hamlet*, Act III, Scene 2:

Ham. Do you see yonder cloud that's almost in shape of a camel?
Pol. By the mass, and 'tis like a camel, indeed.
Ham. Methinks it is like a weasel.
Pol. It is backed like a weasel.
Ham. Or like a whale?
Pol. Very like a whale.

'Irate lady talking to a man in an arm-chair; and a crutch';
'Bear's head, and a hen looking at her reflection in the water';
'Angry beadle ejecting an intruding beaver which has left foot-marks on the floor';
'A man kicking a football';
'Lakes, and green patches of meadow-land';
'Scarecrow behind a young tree';
'Stockinged foot. Bear with a cigarette in its mouth';
'Tiny partridges newly hatched';
'Animal pictures, and the Crown Prince of Germany';
'Smoke going up.'

These were all given for blot 12, and they were given in no spirit of perversity, but seriously, so that the subjects were quite willing to point out how extremely justifiable all their suggestions were.

With blots possessing simpler outlines the variety was commonly one of detail rather than of general idea. For instance blot 1 suggested something of the bird or of the fish to twenty-seven out of thirty-three subjects, but only twelve agreed to call it a duck; three thought it was a goose, two a cock crowing, one a turkey, while three were content to call it a bird without further specification. Similarly with blot 3 which is the most regular in shape of them all. Twelve subjects out of thirty-six called it a seal, six suggested a snail or a slug, four more thought it was a mermaid. The other suggestions were: 'fish' (2), 'whale,' 'young shark,' 'dragon with no legs,' 'cobra,' 'newt,' 'jelly bag lying on its side,' 'Roman lamp,' 'man lying in a sack,' and 'tadpole.'. Two subjects made nothing of it.

With blots more irregular in outline the variety extended to the general situation itself. Two subjects only, out of twenty-three, agreed to call blot 9 a 'lady falling down.' No two persons agreed in their interpretation of blot 12, and the same may be said of most of the blots in the series that were similar in shape to these.

Subjects frequently noticed the very common suggestion of animals. The total number of suggestions obtained was 1068, and of these 635 were of some animal or of a human being. Many of the others were of plants, and in a large number of cases scenes were suggested in which animals or men played a part. Thus inanimate objects came very rarely into the lists. The frequency of animal suggestions was most marked of all in connexion with the blots which were more simple and more of a 'whole' than the others. Here 72 % of the attempts definitely gave animals. Several subjects suggested that the reason was that outlying streamers or points first caught attention. These readily suggested motion and so life. Besides, as one said: "it is living things that are most noticeable and most interesting."

It has of course been noted before that this sort of test may throw a good deal of light on a person's interests and perhaps on his occupation. The subjects themselves often called attention to this. "You ought to be able to tell a lot about a man's interests and character from this sort of thing," they would say. It was a woman, for example, who gave 'bonnet with feathers,' 'blanc-mange,' 'piece of velvet,' 'furs—marabout,' 'piece of shot silk' (twice), 'ostrich feathers,' and 'cross-stitch work,' in her list. The subject who was reminded by one of the blots of "Nebuchadnezzar's fiery furnace, with two men on either side at the top, and two in the middle" was a parson; while the same blot reminded a scientist interested in physiology of "an exposure of the basal lumbar region of the digestive system as far back as the vertebral column up to the floating ribs." Many other illustrations of the same kind of thing might be given.

Apart from special interests, it was common to find that when a subject had once got a certain type of suggestion he would run a good deal on this. One subject saw a man's face in one of the later blots of the series and thereafter to the end he tended, to his annoyance, to see a face in every blot presented. Another, at the end of the trials, said that what struck him most was his tendency "to get a line and then to stick to it." To some extent however this is no doubt due to actual appearance of similarity among the blots themselves. Instances of this kind of persistence of a topic are: 'ghosts,' 'more ghosts kissing,' 'more kissing,' 'green ghosts'; and again: 'picture of an ornamental garden with a colossal statue of a man in evening dress,' 'valley between two hills,' 'valley ending in a bridge,' 'valley seen through a bridge.'

These are the most notable common characteristics. An attempt must now be made at classification based on differences between groups of lists.

This might be done in the common way, by a division of subjects into types. But separation into types, though it is of considerable practical value, solves no theoretical problem. The relatively set ways of reacting which are illustrated in types have practically always been acquired gradually. Whatever has a history presents a problem. And it is no solution to stop the history at some point or other and give a name to what is found. The really interesting thing is to try to show how the different characteristics that are found together in a 'type' are connected, and in what way they have probably grown up. While, therefore, we are unable to do without reference to 'types' whenever a problem of classification arises, the finding names for classes is always merely a preliminary step. It must be followed by a discussion of the relations of characteristics that constitute a particular class, and of the ways in which those characteristics are re-grouped, and perhaps greatly modified, in other classes.

Taking the whole of the results secured, the subjects may first be divided into those (Class 1) whose suggestions were generally particular in nature, with much specification of detail, and those (Class 2) whose suggestions generally referred to no particular object and to no special time. This is not really a hard line of division, because the second class is merely a further product of the same factors that give rise to the first; but it is a sufficiently important distinction to form a basis for classification.

The first class calls for subdivision, for there were: (*a*) those subjects whose lists contained much actual reminiscence, and (*b*) those whose suggestions did not often refer to personal experience, but were nevertheless generally made strictly particular. Both (*a*) and (*b*) represent a stage of imaging proper, but probably the first is more primitive than the second.

No list obtained consisted solely of suggestions belonging to one class, and only a somewhat rough tabulation of results can be given as follows:

Class 1 (*a*):	Subjects dominantly reminiscent	6
Class 1 (*b*):	Subjects occasionally reminiscent and tending generally to definite particularisation	5
	Subjects whose tendency to particularise was marked, but who rarely appealed to memory...	10
Classes 1 and 2 (mixed):	Subjects who either particularised readily and generalised less readily, or generalised readily and particularised less readily	6
Class 2:	Subjects whose suggestions were dominantly general	9

For the subjects in Class 1 (*a*) nearly all blots of course represented particular objects or scenes, and most of them recalled definite events in personal history which were reinstated in visual or other imagery.

Take, for example, these remarks made by one of the 'reminiscent' subjects:

Blot 7 suggested a crown: he said,

"I seemed to be back in the Tower of London looking at the Crown Jewels. I could see the bars in front of them, and the men guarding them. I didn't see myself there, but I *felt* as if I was there. It was Sunday afternoon. You seem to feel different on Sunday somehow, and I felt like that."

Another blot he called two 'robins':

"That reminded me of a picture in a book called *Chatterbox* that I used to look at when I was very young. It is queer because I had really forgotten all about that picture. Now I come to think of it the birds there were not robins at all. But they were standing just as these two are here."

Blot 6 was 'Coto bark':

"That is exactly like a particular specimen of coto bark I saw not long ago. All the circumstances came back vividly to me. I was in the laboratory, and I seemed to see the faces of a lot more men who were there too."

Blot 11 he called a pansy:

"I remembered a Sunday afternoon at Ryde when I was reading a book about a pansy. There was something peculiar about the stigma of this particular flower, and I was specially interested in it. The image of the book came back to me. I could see its green covers. I was sitting in a chair just like this one; with crossed legs, leaning against one of the arms exactly as I am doing now. The author's name came back to me, but not just at first."

This subject had, he said, distinct visual imagery throughout, but never of himself. He came into the scene through having what he called 'the feel of' an experience. This was common in other cases also, particularly when what was recalled had occurred long before. For blot 3, for instance, one of the 'reminiscent' subjects wrote: "Feel shuddery—a conglomeration of slimy snails." Then she said that many years before, at a boarding-house at the sea-side, she had suffered a shock on finding a snail crawling on a bread plate. The feeling she had then was revived when she saw this blot, and became so strong that she had to turn away in disgust. Another subject was reminded by one of the blots of an operation for cancer that he had seen performed long before. He described how he had gone into an anatomical class-room not knowing what was the subject of demonstration. The sudden shock he had then was revived at the sight of the blot.

The feeling was by no means always painful. One 'reminiscent' subject called blot 8 a swallow, and the comment was:

> "That is very curious. I associated the Square here [it was at a small town in Gloucestershire] with that, and it all came back to me how when I was a small boy of say 8 or 9 years I used to run after the swallows, and try to catch them. It was nice to feel that again."

In all these cases it appeared that the first glance reinstated a more or less general situation, marked by a particular feel. Then specific details were developed. In this way suggestions that were not in the shape of the blot, such as suggestions of sound or of particular times and places, came in. Blot 1, for example, was once said to be a "cock crowing early in the morning," and the phrase 'early in the morning' evoked the comment, "Well it has a very aggressive look, and I have been wakened several times lately by a cock-crow early in the morning."

Many subjects however showed a strong tendency to particularisation apart from much personal reminiscence. These often minutely specified their suggestions, dealing much in proper names and adjectives. Such subjects would not say 'rat' simply, but "a particularly venomous rat"; not 'duck' simply, but "the duck I see is standing on its legs running, flapping its wings and quacking"; not simply 'a man walking,' but things like "Peary going to the Pole." Often the suggestions were elaborate, with a kind of dramatic touch about them, as in "Girl leaning over some fence or bridge. Hat falls off. Cape blows back. Scarf flies up like a flag. She falls, screaming"; or "Miser's money bag with long strings. His saucepan ready for the fire by its side"; or "College crest in between two figures that are wrapped in sheets. Napoleon on the top of the crest." Quite often a blot suggested a picture, though not necessarily, as was the case in the proper reminiscent subject, the subject himself looking at the picture. Instances are: "That is like a picture of a camel I've seen in 'Just-so Stories'"; "Very much like a picture I have seen of a genius coming out of a bottle in 'Arabian Nights'"; "It is a sunset with angelic figures coming through the clouds. Just like one of Blake's pictures"; "Turner's 'Angel Faces.'"

Subjects belonging to these first two classes were invariably quick at getting suggestions, very wide in their range, and they appeared highly amused throughout. They laughed readily, not so much at the queer shapes of the blots themselves as at the strangeness of the suggestions that occurred.

But there is a kind of particularisation that is of the blot rather than of the suggestion. In the instances so far considered there was a tendency to forget that something was given, because it was what was suggested that attracted chief attention. But here it was just what was given that was prominent. There may be little or no difference between the actual results secured in the two cases, but the second seems to be farther from imaging proper, and the behaviour of the subjects was distinctly different. In the second case there was much more sense of a problem, much less marked feeling, and in general a rather slower reaction. Very often this kind of particularisation occurred with suggestions that had to do with specialised scientific interests.

In the generalising subjects the feeling that was present seemed to attach not to the suggestions, or to the apprehension of the blots, or to any critical attitude with regard to the shape of the blot, but solely to the *task* itself of getting a suggestion. Subjects reacted more slowly, and there was a larger percentage of cases in which they could not get suggestions at all. What they got was unspecified and stood for any instance of its kind, but no instance was imaged or even thought of.

Most of these points are illustrated in the following complete list which was furnished by a 'generalising' subject[1]:

1 [1]. ——. 2. Bird perched on something. 3. Butterfly. 4. ——. 5 [2]. ——. 6. Potato sprouting. 7 [3]. Tadpole. 8. Bird. 9. Snails. 10. Canary on a perch. 11. Woman's head and shoulders. 12. ——. 13. Moth. 14. Child walking. 15 [7]. Something burning. 16 [8]. Beetroot. 17. ——. 18 [5]. ——. 19. Potato sprouting. 20 [4]. ——. 21. Isle of Skye. 22. Two dancing bears. 23 [9]. ——. 24 [6]. Insect. 25. Two leaves. 26 [10]. A flower. 27. Leaves. 28. ——. 29. ——. 30. ——. 31. ——. 32 [11]. Paw marks. 33 [12]. Footmarks. 34. Sea anemone. 35 [13]. ——. 36. ——.

There was no visualisation with these responses, and with the single exception of suggestion 21 there was no specification. The subject was slow throughout, and took up a thoroughly disinterested attitude. With such subjects there was at times a marked *puzzling* to make out what the blots could represent, and this sometimes aroused a feeling which came near to annoyance.

It is notable that in the above list there is one definite specification: "Isle of Skye" for blot 21. The subject noticed this and said: "It isn't really like the Isle of Syke, but I was talking with a man about that yesterday." The particularisations of the generalising subjects had practically all to do with recent occurrences in this way.

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[1] The numbers given in this list refer to the order of blots as they were presented. The numbers in brackets refer to the reproductions in Plates I and II.

Of the generalising subjects three were students of philosophy who have had much training in abstract thinking, one was a mathematician, and the others were people whose general mode of life is regular and monotonous, who read comparatively little, and whose range of interests is not wide. Of the 'reminiscents' two were women who are accustomed to make up stories for their young children, one was a boy of 9, one a boy of 13, one a youth of 15, and one a man of 22 who had just completed some years training for practical work in chemistry. The best of the particularising subjects were two men and one woman whose interests are dominantly literary and whose reading is wide.

5. DISCUSSION OF RESULTS.

It is now time to attempt some discussion of the bearing of results obtained on the general theory of the nature and relations of perceiving and imaging.

The most impressive feature of the results of the first series of experiments is the great complexity which they showed to be present in much that we call perceiving. This has of course been noticed often enough before. In a "Study of Apperception[1]," for instance, Pillsbury gives a somewhat elaborate scheme according to which we have:

(1) *the sensation*, an element of all cognitive states, and admittedly an abstraction from concrete reality;

(2) *the idea*, a compound or complex of sensations;

(3) *association*, as giving the simplest form of connexion between ideas;

(4) *apperception*, as representing the influence of general experience in consciousness, just as association represents the influence of particular idea upon particular idea;

(5) *perception*, the first concrete conscious process from which all the other forms have been abstracted.

Thus Pillsbury maintains that perception, which he rightly speaks of as within itself a complete act, includes what are called assimilation, associative synthesis, and complication, all the forms, that is, of associative connexion that are discriminated by Wundt. It includes also the apperceptive connexion of apperceptive synthesis, and a good deal of what goes commonly under the name of thinking.

Now undoubtedly perceiving may include all this, but there is no reason for holding that it must. Pillsbury presented words for his subjects to read. His problems were therefore even less simple than some of the earlier of those here given. Both in his case and generally in this, the subjects were adults whose acts are influenced by a mass of distinguishable mental processes. So that, although it is true that all such experiments on perceiving reveal very great complexity, what must be attempted is, not merely the sorting out and naming of distinguishable processes present in the most complex cases, but the decision of what is the minimum complexity possible, and how so large a number of factors come in in the course of development.

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[1] *American Journal of Psychology*, 1896–7, pp. 350–393.

Very often in the mental life it is found that specific processes arise and serve relatively undirected general tendencies. That is the case here. In all instances of perceiving there is what has here been called an 'effort after meaning,' and the many definite processes that may be discriminated within a developed act of perceiving are to be thought of, not so much as constituents of the act, but as ways in which the 'effort after meaning' finds expression in the presence always of direct sense stimulation, for without the latter we cannot speak of perceiving at all.

The term 'effort' does not mean that in all, or indeed in any, acts of perceiving the subject feels himself striving in any way. It is used because in no case is perceiving merely a receiving of something given: there is always some discrimination and selection; and because it is a convenient word to indicate a tendency that while it takes many specific forms retains its general nature throughout[1]. It is by virtue of the latter characteristic that imaging and thinking may both be considered parts of a total act of perceiving in some cases.

The term 'meaning' is equally important. As Hönigswald says, "The factor of meaning (*Sinn*) cuts deeper and deeper into the enormous complexity of the psychic life as the dominating reaction[2]." Here it indicates the presence of at least two directing factors in all attention to a present object: (*a*) the readiness with which an act of apprehension is itself carried through, and (*b*) a certain undefined experience of familiarity in reference to the presented situation. These two are most closely connected, so that often (*a*) depends on (*b*), while (*b*) itself is the direct outcome of the fundamental characteristic of retentiveness. All the various processes of associating and of apperceiving are ways in which the experience of familiarity is rendered definite, and they come to be parts of an act of perceiving because of the persistency of the 'effort after meaning.'

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[1] It has been suggested that in spite of the truth of these considerations 'effort' is not quite the term that should be used. Under ordinary circumstances, it is said, nothing is more effortless than perceiving, and it would be better to use some such phrase as 'tendency to find meaning.' The word 'tendency,' however, is hardly definite enough to name the factor here referred to, and so long as the implication of *strain* is kept away there seems to be no valid objection to the retention of the term 'effort'

[2] *Prinzipien der Denkpsychologie*, quoted by Ewald: *The Philosophical Review*, Vol. XXIII. p. 619: "The circumstance," he goes on, "which seems to me to demand the most serious consideration, even from a purely psychological point of view, is the tendency of everything psychical in the direction of meaning, or what I should like to call simply the 'meaning' of the psychical ...Only the *relatively* meaningless can have a place in the psychic structure; and the striving after a meaningful connexion for elements in an unmeaning juxtaposition probably ceases only with the end of the psychic life itself."

Apart then from the attribution of meaning there is no perceiving, but meaning may be attributed either in a very vague or in a very definite manner. In the former case there is a tendency to speak of the experience as a 'feeling of' something. What the attribution of meaning is at its minimum the experiments throw no light upon, but they do illustrate how, when meaning is vaguely attributed, there is a tendency to speak of it as a feeling. This is the significance of the fact that subjects often said that they "felt" that what they saw represented something, and of the 'feeling of relation' present particularly in connexion with symmetry.

This 'feeling' however is not feeling proper, though it is accompanied by that, but it is really vague apprehension, either in the first place of some *thing* or in the second place, when analysis begins, of certain relations. By development of the first we get all the marks of imaging, and of the second all the marks of thinking. The two, in fact, proceed together.

So far as these processes of development are met with at all in experiments they are already in mid-course. A simple situation—a diagram or a picture—is presented, and is seen as a whole. But there is always a tendency to see it as an instance of something, and this may be so clear that the subject gives the object a name. Definite naming, of course, requires that much work of analysis and synthesis, much recognition of likeness amid difference, shall have been carried out, and so itself indicates considerable development. But, as in these experiments, there are plenty of cases in which a subject, without definitely giving a name, will say that he has an impression that what is presented is an instance of something. This impression seems capable of analysis into (*a*) an apprehension of what is given, and (*b*) a feeling not quite definite enough to be called a feeling of familiarity, but rather merely of the relative ease with which the act of apprehending is carried out. The latter is a true feeling, attaching to the whole act, and not a vague apprehension—called a feeling only because of its vagueness—of relations of parts within an object. This feeling may have a good deal to do with the influence of general experience upon perceiving, and at any rate is at the foundation of much imaging.

For all acts of perceiving yield the familiar analysis into act, object and content, the relation of act and object in this case being brought about always by means of sense experience. Specifying the content, and accompanying the whole act, is a feeling which may originally be one merely of relative ease or hesitation, but which very rapidly takes certain quite definite forms, and in these experiments appears as satisfaction, familiarity, surprise, disgust, and so on. It is the feeling which, upon a repetition of the same general situation, plays a leading part in tending to reinstate the content. But at first, since the feeling qualifies the act as a whole, it is the whole content in all its particularity that tends to be reinstated. The image proper is as concrete and individual as the percept. In the last of the series here employed all the subjects in whom imaging was most clear and precise were persons with dominantly concrete interests, and in many cases the imaging was induced largely by some particular feeling, together of course with the apprehension of what was presented.

But though the content, in an act of imaging proper, is always capable of precise localisation, at the same time it is far more fluid than that of an act of perceiving. In perceiving we say "that *is* so-and-so," but in such cases of imaging as we have in the last series we say commonly "that *may be* this, or this, or something else." Moreover transpositions in imagery are much more common than are generally supposed. Consequently images are all the time tending to lose their particularity.

The basis of this is the development of analysis which gets its earliest illustration, so far as the present experiments go, in the 'feelings of relation.' As has been said already these are not feeling proper at all. They do not simply accompany an act, but indicate rather that the act is now directed, not upon the object as a whole but upon parts of it, so that the content holds together detail, at first vaguely, but then with more and more definiteness. And this apprehension of relations is the basis of the generalisation which is the chief mark of thinking[1].

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[1] P. 14. See also Höffding: *Modern Philosophers and Lectures on Bergson* (Eng. trans.), pp. 256–7. Höffding uses the term "analytical intuition," and points out that Descartes and Poincaré have described it, and that "it designates for them the very passage from perception to analysis or proof."

For this analysis, indefinite as it is, means that a part of what is presented, having relations to other parts, may, on its recurrence, constitute for the subject much the same general situation as would the recurrence of the whole. And then what is not presented to sense, since nevertheless it is reinstated for experience, is already relatively free from a set context. But the constant change of life continually presents relations of sameness, difference, relative position, and so on, with immense variation of material; and so it comes about that the recurrence, for sense experience, of parts of an object tends to reinstate, for imaging, wholes very different in character. This itself tends to damp down the feeling characteristic, for feeling conspicuously attaches itself to particular wholes. At first, when suggestive material is presented, as in Series 4 and 5 here, this often induces the subject to say: "That is this, or this, or this"—there is a rapid alternation of suggestions. But all the while the processes of analysis are going on, the relations of the various suggestions are noted, their points of similarity are grouped together, and the suggestions narrow to a whole, relatively unqualified by feeling, which is itself capable of much specification. This is the stage of the generalising subject of the last series, and comes far nearer to thinking than to imaging proper. To fix both specifications and generalisations naming is important, and hence the prominent part it played throughout the experiments.

In a complete act of perceiving then, imaging, as the reinstatement of a situation or object not presented wholly to sense experience, and thinking, in its beginnings as an apprehension of relations apart from the nature of what is related, may both be found. They both involve the partial freeing of a content from sense background, and this may go farther, so that the processes take place in the absence of sense stimulation altogether. Imaging however always tends to retain a characteristic definiteness of content, and to occur in close connexion with strongly marked feeling. Thinking may achieve equal or greater definiteness, but the definiteness always appears as a characterisation of that which is thought about, and feeling is at a minimum.

If this analysis is on the right lines, then all attempts to find original constituents of mental life are wrong-headed. There are doubtless specific feeling-attitudes of, say, satisfaction, surprise, disgust and so on. These, so far as they are thus definite, appear all to have developed out of feeling-qualification that is much more vague, and the experiments show how quickly they tend to develop. But in any case they are not material out of which the mental life is made. They are found alike with perceiving, with imaging, with thinking, with willing, with any kind of reaction of a conscious subject to environment, and are simply ways in which such reaction is qualified.

Similarly there are specific ways in which acts of perceiving, of imaging, and of thinking may proceed, and some of these have been given in the summary of results in the earlier part of this record. But the acts themselves are no more made up of them than they are made up of those features in the objects themselves that facilitate perceiving or imaging. We have no right whatever to speak of original sensations, or percepts, or images, or ideas from the putting together of which a man is made. When, for instance, Karl Bühler, finding good evidence for the existence of acts of thinking entirely unaccompanied by images, speaks of elementary *Gedanken*, and cites in illustration the appreciation of *Gestaltqualitäten* which, he thinks, proceeds in early cases by an intellectual apprehension of the meaning (*Sinn*) of a presented figure, what he has found is that in a total act of perceiving there already may be the beginning of the process of analysis. That is true enough. But if then we go on to speak of original specific feelings, of original distinct percepts, and of original thoughts, and try to build them together into a life, we are hopelessly astray.

At the beginning of this paper some of the difficulties of the search for the simple were mentioned. It is now clear that what we want is not a number of simple elements out of which at the most we could construct a toy for psychologists to play with, but a knowledge of the minimum of conditions under which an effort after meaning of a specific character can take place, and further of the ways in which such distinguishable efforts are themselves related.

6. SUMMARY.

We see then that under many different conditions, and in many different forms, the fundamental 'effort after meaning' is found. And we have to try to say what form is present under what conditions. At first, always, there must be immediately present sense experience, and then we have an act of perceiving. This however itself takes place under many diverse circumstances, and some of the forms of perceiving and their conditions were brought out in the early parts of the paper. Largely, however, through the feeling that accompanies all effort to attribute meaning, and through the analysis of which this feeling may be the basis and which is found anyhow in the perceiving of all developed subjects, we soon come to be able to go beyond immediate sense experience, though that is a complication which at once saves time and increases the chance of error. Then we get, first imaging, in such forms and under such conditions as were illustrated in the second part of the paper, and eventually thinking, all three processes being commonly closely connected. In imaging feeling proper plays a dominant part, and as this tends always to be attached to a whole situation, the content of an act of imaging always tends to be particular and capable of precise localisation. But then, perhaps mainly through lack of variety of experience, or maybe by the great growth of a positive interest in analysis—for these two may produce substantially the same results when these results are viewed merely from without—imaging tends to become less and less definite, and more and more general. At that stage this investigation ceases. Before any definite study of the conditions and forms of thinking proper can be made, it will be necessary to consider the evidence for the fluidity of the content of imaging, and the various forms of transposition that imaging constantly illustrates, and that lead to all kinds of conventional representation[1].

There is one other point. In the feeling which plays a prominent part in freeing content from immediate background of sense, and so in determining early forms of imaging we have the foundation, probably, of all criticism and valuation. Yet it is only in thinking proper, where we are commonly away from the particular, and so freer from feeling than we are under any other circumstances, that criticism is usually considered to reach its highest development. Here also is a problem for further study.

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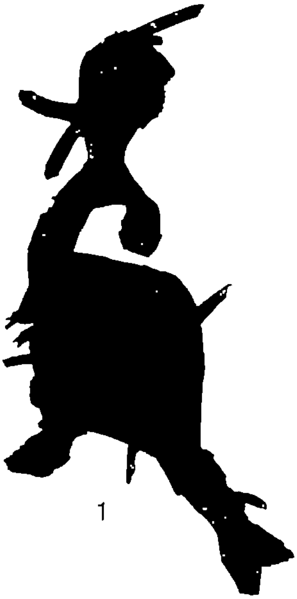
[1] Cp. Philippe: "Sur les Transformations de nos Images Mentales," *Revue Philosophique*, 1897, pp. 481–93: "L'image est mobile," he says, "et vivante, et soumise à de perpétuels changements sous l'incessante action de nos sentiments ou de nos idées." Philippe indicates certain common general ways in which images undergo transformation, but his study is preliminary only, and the whole subject calls for more detailed experimental investigation.

(*Manuscript received October* 1915.)



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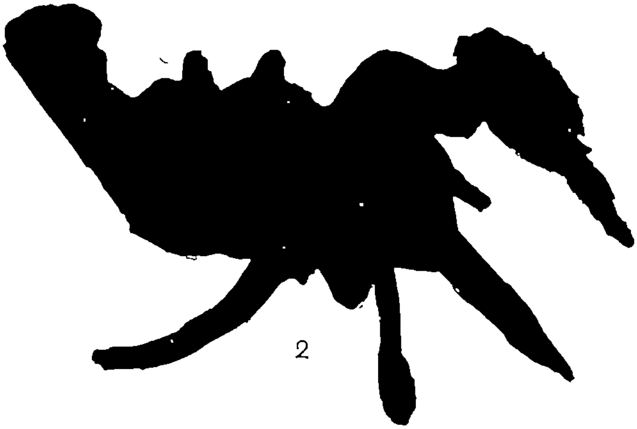

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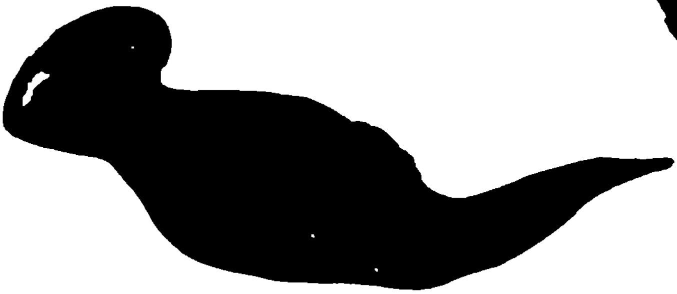

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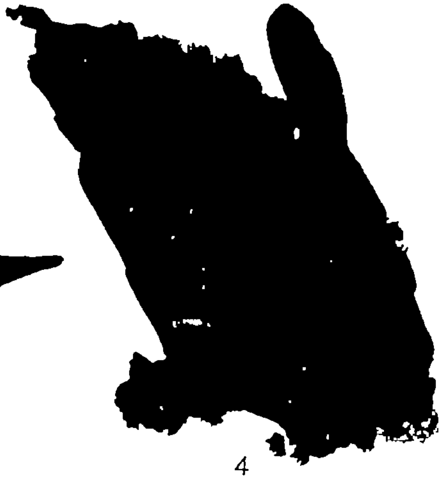

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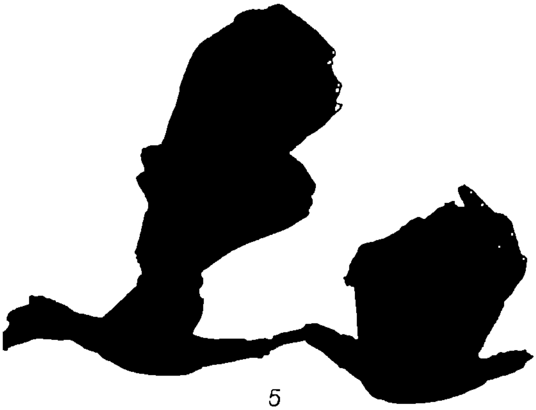

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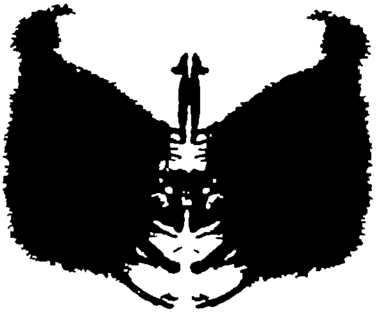

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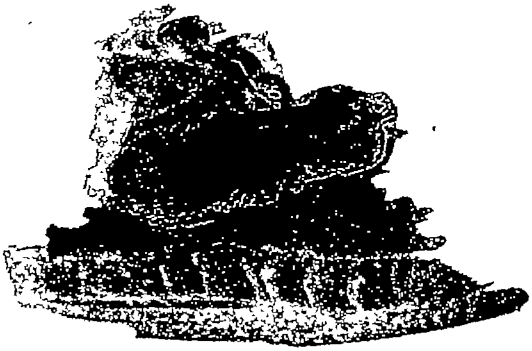

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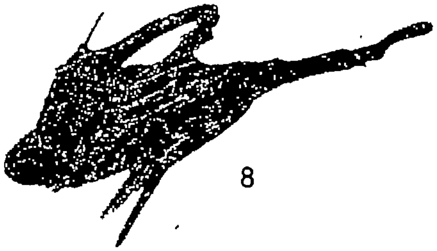

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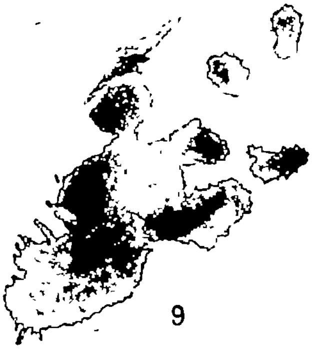

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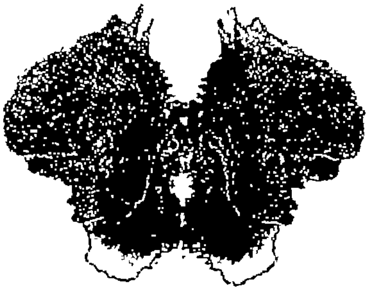

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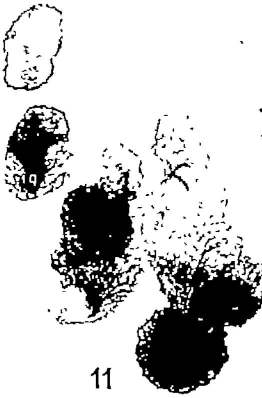

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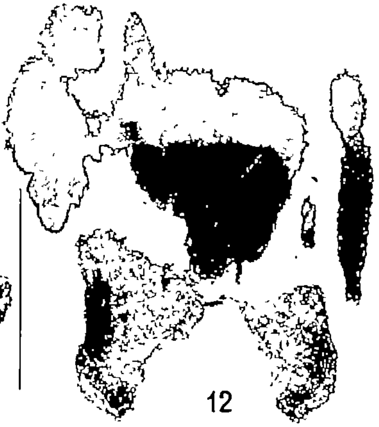

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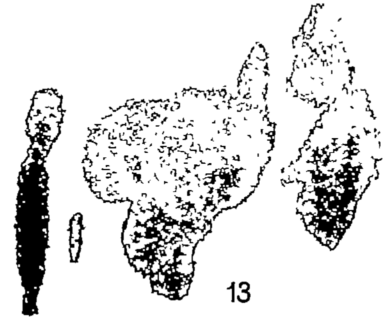

13

Chromo-lith Cambridge University Press

PUBLICATIONS RECENTLY RECEIVED.

Bodily Changes in Pain, Hunger, Fear and Rage: an account of recent researches into the Function of emotional excitement. By WALTER B. CANNON. New York and London: D. Appleton and Co. 1915. Pp. xii + 311. $2.

Professor Cannon has performed a most useful task in collecting and presenting systematically in book form a summary of the brilliant investigations undertaken by himself and his collaborators during the last four years at the Harvard Physiological Laboratory. The main outcome of the researches is to show that the physiological changes which accompany painful stimulation or intense emotional excitement have considerable biological value. From a brief account of the autonomic nervous system it is shown that, within the same viscus, an antagonistic relation holds between the innervation effects of the mid-division (sympathetic branch) of the system and those of either of the end-divisions (cranial and sacral branches). Hence temporary dominance of, say, the impulses transmitted by neurones of the sympathetic branch tends to overwhelm, for the time being, the activity of the related cranial and sacral branches. Fear, rage and stimulation which in the unanaesthetized subject would produce pain, alike seem to promote the supremacy of the sympathetic division. They are, too, all accompanied by increased activity of the adrenal glands, whose action (as is shown at length in chapters III—X) is similar to that of the neurones of the sympathetic division. Among the important effects of adrenal stimulation are the liberation of sugar from the liver into the blood-stream, the relaxation of the smooth muscles of the bronchioles, the counteractions of muscular fatigue, the forcing of the blood out of the abdominal viscera to the heart, lungs and central nervous system and more rapid coagulation. None of these changes, it will be observed, are of a conservative character; they denote instead an increased expenditure of energy and a mobilizing of reserves—to this end even the functions of the alimentary canal are temporarily suspended. It is here that the importance of hunger can be seen, since its function seems to be that of insistence on the necessity for the dominance of the cranial division. Hunger can be only temporarily suspended. Antagonism between sacral and sympathetic innervation is not always quite so obvious or clearly defined. It is true to assert as regards many of the organs innervated by both sacral and sympathetic branches that increase of tone is brought about by the latter, decrease of tone by the former. On the other hand, in very intense periods of emotional stress (*e.g.* great fright), when the sympathetic division is strongly innervated, there definitely appears to be a clear supremacy of the sacral innervation of the bladder and distal colon. To explain this the suggestion is made that the 'orderliness of the central arrangements is upset' so that the 'opposed innervations no longer discharge reciprocally but simultaneously,' the stronger member of the pair prevailing. Antagonism between well-marked emotional states of either division is, however, incontrovertible. It is therefore possible to explain in physiological terms, to some extent, the inhibition of one instinct by another, *e.g.* the inhibition by fear or anger of the sexual instinct.

According to Prof. Cannon the visceral accompaniments of the two strikingly unlike major emotions, fear and anger, do not differ in any noteworthy respect;

and further, every feeling if sufficiently intense appears to produce almost uniform visceral responses. The James-Lange theory, therefore, finds no support. This identity of response suggests the possibility of substitution, and in the final chapter it is claimed that international athletic competitions would serve to keep alive and to satisfy the beneficial features of the fighting instinct and thus provide a suitable biological alternative to war.

E. M. S.

A Point Scale for Measuring Mental Ability. By R. M. YERKES, J. W. BRIDGES, and ROSE S. HARDWICK. Warwick & York, Inc. Baltimore. 1915. Pp. 218. $1.25.

Probably this book presents the most important modification of the Binet scale yet published. The chief alteration is in the method of scoring. A scale of marks for each test supplants the 'all-or-none' method of scoring used by Binet. Instead of groups of tests for various mental ages, the age norms are expressed in terms of total number of marks gained by standard groups. The marking of the tests has been so arranged that the possible maximum number of marks is 100. All the twenty tests included in the scale are Binet tests (with slight modifications) with one exception,—the 'analogy' test. The Binet tests which have been omitted are those which are chiefly tests of specific instruction, *e.g.* money values, months, date, etc., and these omissions seem justified by the experience of most persons who have used the Binet scale. The authors have established age-norms with their scale by the examination of about 750 boys and girls. The norms are conveniently presented diagrammatically. In addition to curves giving the norms established with heterogeneous groups, there are norm-curves for English-speaking (*i.e.* born to the English language) boys, English-speaking girls, non-English-speaking boys, and non-English-speaking girls. The results obtained by the use of the Point Scale are compared with those obtained with the Binet scale. The reviewer, also, has made a similar comparison with 24 mentally-defective-pupils,—admittedly too small a number for purposes of generalisation: and it was found that 19 pupils scored a higher mental age, 2 the same, and 3 a lower mental age with the Point Scale than with the 1911 Binet scale. The difference with some of the pupils amounted to two years.

Notwithstanding several obvious advantages of this modification of the Binet scale, the authors admit that they already see the possibility of abandoning it in favour of a better one. In fact, the last chapter of the book deals with proposals for a universal point scale to be applied to a larger range of ages, and one which is likely to prove an altogether more comprehensive scale than anything hitherto formulated.

E. O. L.

Instincts of the Herd in Peace and War. By W. TROTTER. London: T. Fisher Unwin. 1916. Pp. 213. 3*s*. 6*d*. net.

A very original and suggestive essay admirably written. Mr Trotter applies Biological (or Comparative) Psychology to social affairs. That man is a social animal has often been recognised; but the consequences of this fact have never been scientifically followed out. One consequence is that the beliefs of the herd, dictated by its instincts, are accepted as axiomatic by its members, who are thus prevented from seeing the facts and from learning by experience. The control of social affairs falls into the hands of those who are most easily satisfied with the herd-beliefs; although for that very reason they are blind to all the shortcomings (often outrageous) of the social state. They are called "normal," but better "resistive," because of their attitude towards new ideas and movements. On the other hand, with the growth of civilization, there is a great increase of individuals of another type, in whom there exists an irreconcileable conflict between the beliefs of the herd and the teachings of experience. Such people are unstable and often stigmatised as "degenerate." The opposition between these types can only be overcome by such a development of society that instinctive beliefs which are not true shall be supplanted by rational experiential knowledge, and to attain this there must be conscious direction But there are three types of gregariousness: the aggressive type of the wolf, the defensive type of the sheep and ox, and the socialised gregariousness of the bee. In our present unhappy circumstances, we cannot help seeing that Germany has undertaken the necessary task of consciously directing all state activities, but has at the same time been driven to adopt the wolfish type of organization. England, by the more favourable trend of her history, has fallen into the socialised gregariousness of the bee, but without conscious direction. and, consequently, with endless waste and confusion. Nevertheless, socialised gregariousness is the necessary goal of evolution.

Perhaps one glance at history and another at the map of the world may make the reader doubt whether England is free from all traces of the wolfish disposition. Favourable circumstances have in some cases, and happily in our own, mitigated that disposition which is common to mankind, and have "sublimated" the wolfish instincts by directing their energy into new openings for pursuit and emulation. But the narrow space given to reviews in this *Journal* prevents the raising of the few objections that may occur to one, and also the illustration of the many remarkable reflections to be found in Mr Trotter's book and the broad spirit of humanity that pervades the whole of it.

C. R.

Mind, Nos. 96, 97 and 98. London: Macmillan and Co. 1915, 1916. 4*s*. each.

The Philosophical Review, Vol. XXIV, No. 6, Vol. XXV, Nos. 1 and 2. New York: Longmans, Green and Co. 1915, 1916. 3*s*. net.

The Journal of Animal Behaviour, Vol. V, Nos. 4, 5 and 6 Cambridge, Boston, Mass.: Henry Holt and Co. 1915. Subscription, $3.00 per volume.

Bulletin de l'Institut Général Psychologique, 15^{e} Année, Nos. 4—6. Paris: Au siége de la Société. 1915. Abonnement annuel, 20 f.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

Nov. 20, 1915.	The Part played by Ejective Cognition in the Development of Self-Consciousness, by Miss O. A. Wheeler. The Psychical Effects of Shell Shock, by F. W. Mott. An Enquiry into the Factors determining Change in Illusions of Reversible Perspective, by Miss L. G. Fildes.
Jan. 29, 1916.	The Theory of Symbolism, by Ernest Jones. On the Relation of Magic and Animism, by Carveth Read.
March 25, 1916.	Mind in Man and Monkey, by Carveth Read. On the Nature of Pain, by A. Wohlgemuth.

VOLUME VIII SEPTEMBER, 1916 PART 3

A HIERARCHY WITHOUT A GENERAL FACTOR[1].

BY GODFREY H. THOMSON.

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1. *Introduction.*
2. *An Extension of Weldon's Experiment.*
3. *Dice-throwing Experiments to imitate Specific, Group, and General Factors.*
4. *A Hierarchy without a General Factor.*
5. *Application of Professor Spearman's Criterion.*
6. *Experimental Values.*
7. *Conclusion.*

1. *Introduction.*

THE object of this paper is to show that the cases brought forward by Professor Spearman[2] in favour of the existence of General Ability are by no means 'crucial.' They are it is true not inconsistent with the existence of such a common element but neither are they inconsistent with its non-existence. The essential point about Professor Spearman's hypothesis is the existence of this General Factor. Both he and his opponents are agreed that there are Specific Factors peculiar to individual tests, both he and his opponents agree that there are Group Factors which run through some but not all tests. The difference between them is that Professor Spearman says there is a further single factor which runs through all tests, and that by pooling a few tests the Group Factors can soon be eliminated and a point reached where all the correlations are due to the General Factor alone.

The proof advanced for this hypothesis rests upon the possibility of forming a 'hierarchy' with the correlation factors between a number of tests taken in pairs (what is meant by a hierarchy will be explained immediately). If a hierarchy can be formed the existence of a General Factor is said to be proved. Such a statement is however incorrect or at least misleading and it is to this point that my paper is devoted. I propose to show that an excellent hierarchy can be made with Specific and Group Factors only, without a General Factor.

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[1] This paper was prepared in 1914 but I requested that its publication should be delayed as both Major Spearman and myself became engaged in military work. There appears however to be no advantage in further delay and after correspondence with Major Spearman I have decided to put it forward. G. H. T.

[2] This *Journal*, v. (1912), and elsewhere.

TABLE I.

	a	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*k*	*l*
a		867 867	727 730	600 593	372 356	186 174	162 167	138 120	134 116	110 112
b	867 867		665 650	549 550	340 341	170 143	148 137	126 088	122 084	100 082
c	727 730	665 650		461 500	286 292	143 143	129 091	106 088	102 084	084 041
d	600 593	549 550	461 500		235 244	118 095	106 091	087 088	084 042	069 041
e	372 356	340 341	286 292	235 244		073 093	065 089	054 043	052 041	043 040
f	186 174	170 143	143 143	118 095	073 093		033 044	027 042	026 040	022 039
g	162 167	148 137	129 091	106 091	065 089	033 044		024 040	024 039	020 037
h	138 120	126 088	106 088	087 088	054 043	027 042	024 040		020 037	016 036
k	134 116	122 084	102 084	084 042	052 041	026 040	024 039	020 037		015 034
l	110 112	100 082	084 041	069 041	043 040	022 039	020 037	016 036	015 034	

The Upper Hierarchy contains a General Factor = 19 The total number of factors in each test is

a	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*k*	*l*
20	24	34	50	130	520	650	940	1000	1500

The Lower Hierarchy contains *no* General Factor. The average difference is 0·016. To bring the average p.e. down to anything like this, over 1000 cases would be required.

In a hierarchy such for example as either of the two given in Table I every coefficient is larger than its neighbour on the right and its neighbour below it. For a perfect hierarchy not only should this be the case but there should exist a constant ratio between all the numbers in any given pair of columns. In such a perfect hierarchy the coefficient of correlation between any pair of columns will be unity. Professor Spearman has shown that a General Factor will, in the absence of Group Factors, produce such a perfect hierarchy. In practice there will necessarily be sampling errors, and in practice no hierarchy can be expected to be perfect. The question then arises how great a deviation from perfection we can allow, and still believe it probable that a General Factor exists, and no Group Factors.

The answer to this question of course depends on a further consideration, viz., what likelihood is there of obtaining a hierarchical arrangement in the absence of a General Factor? Professor Spearman has considered this point and answered it as follows.

If none but quite Specific Factors are present, the correlations will all be zero, and the pairs of columns will show no correlation with one another. If however correlations exist, but are due to Group Factors alone, then tests which share a Group Factor will correlate highly, but others will not correlate at all. Let there be three such Group Factors; then we shall obtain not a hierarchy but an arrangement like this:

.	S_1	S_2	S_3	A_1	A_2	A_3	D_1	D_2	D_3
S_1	.	*h*	*h*	*l*	*l*	*l*	*l*	*l*	*l*
S_2	*h*	.	*h*	*l*	*l*	*l*	*l*	*l*	*l*
S_3	*h*	*h*	.	*l*	*l*	*l*	*l*	*l*	*l*
A_1	*l*	*l*	*l*	.	*h*	*h*	*l*	*l*	*l*
A_2	*l*	*l*	*l*	*h*	.	*h*	*l*	*l*	*l*
A_3	*l*	*l*	*l*	*h*	*h*	.	*l*	*l*	*l*
D_1	*l*	*l*	*l*	*l*	*l*	*l*	.	*h*	*h*
D_2	*l*	*l*	*l*	*l*	*l*	*l*	*h*	.	*h*
D_3	*l*	*l*	*l*	*l*	*l*	*l*	*h*	*h*	.

h = high correlation. *l* = low correlation. See this *Journal*, v. (1912), p. 57.

in which the high correlations are concentrated along the diagonal. In this arrangement some columns will correlate positively, namely those in which the high correlations come opposite one another; but these will be in the minority and most pairs of columns will correlate negatively. Professor Spearman concludes therefore that in the absence of a General Factor the average correlation between columns will be either zero or negative, and that only a General Factor will give a very high positive correlation between pairs of columns.

In this consideration of Group Factors however Professor Spearman has tacitly assumed that there is no overlapping of such factors. If this were so then indeed a hierarchy would be impossible. But it is

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at any rate a conceivable hypothesis that such overlapping should occur, that for example there might exist a factor common to three tests a, b, c and another common to c, d, e, so that c contains both factors: and on this hypothesis an excellent hierarchy can be obtained without any General Factor, and the average column correlation can even approach unity.

In order now to avoid all psychological controversy I propose to deal with entirely non-psychological material and to apply Professor Spearman's formulae to dice throwing. To do so it is first necessary for me to consider an extension of Weldon's well-known experiment.

2. *An Extension of Weldon's Experiment.*

A description of this experiment is easily accessible in Dr Brown's *Mental Measurement*. If a number of dice be thrown and the score taken, and then half of them (which for convenience are red) be left lying and the remainder rethrown, then clearly the red dice are common to both throws, and in fact if the correlation between a number of such pairs be found it proves to be one-half. More generally, as Dr Brown shows, if n dice be thrown, l left lying and the remainder rethrown then the correlation is

$$r = \frac{l}{n}.$$

A still more general case not given by Dr Brown is as follows. Let $l + m$ dice be thrown for the first throw, l left lying, and k thrown to form with l the second throw of $l + k$ dice. Here l dice are common 'factors' and the correlation r will lie between $\frac{l}{l+m}$ and $\frac{l}{l+k}$. Its exact value can be found as follows. Let the successive scores be

$$\begin{array}{c}
a_1 + \boxed{c_1, \qquad c_1} + b_1, \\
a_2 + c_2, \qquad c_2 + b_2, \\
a_3 + c_3, \qquad c_3 + b_3, \\
\cdots\cdots\cdots \qquad \cdots\cdots\cdots \\
\cdots\cdots\cdots \qquad \cdots\cdots\cdots \\
a_N + c_N \qquad c_N + b_N,
\end{array}$$

then if $x = a + c$ and $y = c + b$,

we have
$$r_{xy} = \frac{\sigma_x^2 + \sigma_y^2 - \sigma_{x-y}^2}{2\sigma_x\sigma_y}.$$

But
$$\sigma_{x-y}^2 = \sigma_a^2 + \sigma_b^2,$$
$$\sigma_x^2 = \sigma_a^2 + \sigma_c^2,$$
$$\sigma_y^2 = \sigma_b^2 + \sigma_c^2,$$
whence
$$r_{xy} = \frac{2\sigma_c^2}{\sqrt{(\sigma_a^2 + \sigma_c^2)(\sigma_b^2 + \sigma_c^2)}}.$$
Now
$$\sigma_c^2 : \sigma_a^2 : \sigma_b^2 :: l : m : k,$$
so that
$$r_{xy} = \frac{l}{\sqrt{(l+m)(l+k)}}.$$
This result[1], which I have tested by experiment, seems to be of considerable interest in connection with the significance of the correlation coefficient.

Here however I cannot dwell on this point. At present I only need the above formula for the purposes of the next paragraph.

3. *Dice-throwing Experiments to imitate Specific, Group, and General Factors.*

In psychological tests a boy John (say) is tested in perhaps ten ways and gets a numerical score for each test. Let us imitate this with dice. Note that I do not for one moment suggest that psychological 'factors,' if they exist, can be added together like dice: I merely intend to apply Professor Spearman's formulae to dice throwing. If we throw an entirely fresh set of dice to represent each of John's ten tests, then clearly the factors are entirely specific, there will be no correlation whatever between the scores in the ten tests.

If some of the dice are red, and these red dice are left lying and counted in to every score, there will be a General Factor. The red dice will of course be rethrown when we are finding the scores of the next boy, in whom the General Factor may be greater or less. The number of white dice thrown will vary from test to test to represent the Specific Factors in each test. The correlations in this case will form a perfect hierarchy. For example the upper hierarchy in Table I could be obtained in this way. The numbers there given are the theoretical values calculated by
$$r = \frac{l}{\sqrt{(l+m)(l+k)}}.$$
The number of dice common to all the tests is 19, and the total number of dice in each test is shown below the Table.

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[1] I am pleased to see from Professor Spearman's comments which follow this paper, that this result is deducible from a previous formula of his.

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Notice however the lower numbers. They also form a hierarchy: not, it is true, a perfect hierarchy, for the relationship $\frac{r_{ap}}{r_{bp}} = \frac{r_{aq}}{r_{bq}}$ is not everywhere satisfied; but nevertheless a very good hierarchy with no reversals, that is the numbers steadily decrease from the N.W. corner to the S.E. Even the few cases where the successive numbers are equal instead of decreasing could have been avoided if I had cared to face a little more labour in the construction and calculations. This hierarchy however, which apes so closely the former, contains no General Factor whatever. Its construction is described in the next paragraph.

4. *A Hierarchy without a General Factor.*

Fig. 1 shows an arrangement in which ten tests depend upon 145 factors, of which 109 are quite specific and only occur in one test each, while the remaining 36 are group factors which run through more than one test each, but never through all. The distribution of these 36 group factors is shown in the figure. Most of them run through only two, three, or four tests, three of them run through five tests, but not one runs through more than five tests out of the ten. There is therefore nothing approaching a General Factor. The number of purely Specific Factors in each test is also given.

In obtaining actual scores with dice thirty-six dice, marked so that each was recognisable, would be thrown, and the score of each placed in the proper place in Figure 1. These are the Group Factors. Then in addition the dice representing the Specific Factors would be thrown. These would be entirely separate for each test—for example fourteen dice would be thrown to complete test *f*. The scores of the various tests could then be added up and the totals would be analogous to the scores of a single boy in the ten tests. The whole would then be repeated for each other boy.

To make this quite clear consider two tests in detail, say tests *d* and *e*. For test *d*, twenty dice in all have been thrown. Of these five are left lying and sixteen other dice thrown to complete test *e*. The correlation between test *e* and test *d* will therefore be by the formula found above

$$r = \frac{l}{\sqrt{(m+l)(l+k)}} = \frac{5}{\sqrt{20 \times 21}} = 0{\cdot}244.$$

By the same formula the correlations between all the tests in pairs can be found. They are the lower numbers already shown in Table I and form as has been said an excellent hierarchy. So excellent indeed, as to be indistinguishable from a perfect hierarchy in any actual experiment unless many more subjects were examined than has ever yet been the case. This will be shown in the two following sections of the paper.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
a	×	×	×	×	×	—	×	×	×	×	×	×	×	×	×	—	—	—
b	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	—	—	—
c	×	×	—	×	—	×	×	—	—	×	×	—	—	×	—	—	—	—
d	×	—	×	—	×	—	—	×	—	—	—	×	—	—	×	×	—	—
e	—	×	—	—	×	—	—	—	×	—	—	—	×	—	—	—	×	×
f	—	—	×	—	—	—	×	—	—	×	—	—	—	—	—	—	×	—
g	—	—	—	×	—	—	—	×	—	—	—	—	×	—	—	—	—	—
h	—	—	—	—	—	×	—	—	—	—	—	×	—	—	—	—	×	—
k	—	—	—	—	—	—	—	—	×	—	—	—	—	×	—	×	—	—
l	—	—	—	—	—	—	—	—	—	—	×	—	—	—	×	—	—	×

	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	*S*
a	—	—	—	—	×	—	—	×	—	×	×	×	—	×	×	×	×	×	0
b	—	—	—	—	—	—	—	—	—	—	—	—	—	×	×	×	×	×	0
c	—	—	—	—	—	×	×	×	—	×	×	×	—	×	×	×	×	×	1
d	—	—	—	—	—	×	×	×	×	×	—	—	—	×	×	×	×	×	3
e	—	—	—	—	—	×	—	—	—	—	×	—	×	×	×	×	—	—	9
f	×	×	—	—	—	—	—	—	×	—	×	—	—	—	—	—	—	—	14
g	×	—	×	×	—	—	—	×	—	—	—	—	×	—	—	—	—	—	16
h	—	—	×	—	×	—	—	—	—	×	—	—	—	—	—	—	—	—	20
k	—	×	×	—	—	—	—	—	—	—	—	×	—	—	—	—	—	—	22
l	—	×	—	×	×	—	—	—	—	—	—	—	—	—	—	—	—	—	24

Fig. 1. Distribution of Group Factors.

a, b, c, etc. are the names of 'tests.'

1. 2, 3, etc. are 'factors.'

For example factor number 15 (perchance 'visual memory') runs through tests *a, b, d, l.*

In addition to the Group Factors there are Specific Factors, the number of which in each test is indicated under *S*.

5. *Application of Professor Spearman's Criterion.*

Consider first the average difference between the coefficients of the two hierarchies. It is only 0·016. Mr Burt in his paper[1] compared the average probable errors of his coefficients with the average difference between his experimental numbers and the theoretical numbers of a perfect hierarchy. But to reduce the average probable error of an experimental determination in our case down to anything like 0·016 more than one thousand subjects would be needed. That is, until this number had been examined we could not even begin to guess from which of these hierarchies our experimental numbers were derived.

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[1] This *Journal*, III. (1909), p. 159 ff.

Clearly too the correlations between the columns of the imperfect hierarchy are very high. The uncorrected correlation between the earlier columns is more than 0·99 in a large number of cases and even the worst correlation, between columns k and l, is 0·88. The average is therefore undoubtedly very high and positive, and there is no doubt that an experimental determination of this hierarchy would furnish apparent proof, according to Professor Spearman's formulae, of the existence of a General Factor, especially as in such an actual case the lower columns would be unlikely to reach his 'correctional standard,' and only the upper columns would be used. Such an experimental determination I have carried out, assisted in the dice-throwing by a number of my students, whom I here sincerely thank for the trouble taken and time spent in that monotonous occupation.

6. *Experimental Values.*

We threw in all 5220 dice, in 36 groups of 145 each, to represent ten tests in a class of 36 boys. The numbers were entered on forms according to Figure 1, the totals obtained, and the marks then treated exactly as marks in tests. The correlational coefficients actually obtained are shown, with their probable errors, in Table II. To this hierarchy I have applied Professor Spearman's criterion in its entirety. His corrected formula for column correlation is

$$R_{ab}' = \frac{S\,(\rho_{xa}\rho_{xb}) - (n-1)\,r_{ab}\,\overline{\sigma_{xa}\sigma_{xb}}}{\sqrt{S\,(\rho_{xa}^2) - (n-1)\,\overline{\sigma_{xa}^2}}\,\sqrt{S\,(\rho_{xb}^2) - (n-1)\,\overline{\sigma_{xb}^2}}}\,,$$

in which the ρ's are the coefficients r measured from the mean of the column, and the σ's are the probable errors of the r's, divided by ·6745. The bar indicates mean values.

It is laid down further that only those pairs of columns may be used in each of which $S\,(\rho^2)$ is at least twice its correction $(n-1)\,\overline{\sigma^2}$. In our hierarchy four pairs of columns reach this standard, and for these we have

$$\begin{array}{lll} \text{for} & ab & R' = 1{\cdot}04 \\ & ac & \quad\; = 1{\cdot}00 \\ & bc & \quad\; = 1{\cdot}01 \\ & cd & \quad\; = \underline{1{\cdot}11} \\ & \text{Mean} & \quad\;\;\; \underline{1{\cdot}04} \end{array}$$

Our hierarchy therefore triumphantly passes the test, *and would be added, by anyone ignorant of its real composition, to the list of those already considered by Professor Spearman to contain a General Factor.*

TABLE II.

	a	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*k*	*l*
a		823 036	703 057	708 056	367 097	337 100	281 103	371 097	112 111	133 110
b	823 036		732 052	679 060	287 103	338 100	209 108	254 105	183 109	222 107
c	703 057	732 052		560 072	317 101	117 111	222 107	180 109	043 112	286 103
d	708 056	679 060	560 072		293 103	187 109	344 099	406 094	205 108	181 109
e	367 097	287 103	317 101	293 103		-059 112	080 111	-062 112	012 112	090 111
f	337 100	338 100	117 111	187 109	-059 112		-031 112	330 100	-054 112	314 101
g	281 103	209 108	222 107	344 099	080 111	-031 112		431 092	228 107	031 112
h	371 097	254 105	180 109	406 094	-062 112	330 100	431 092		135 110	166 110
k	112 111	183 109	043 112	205 108	012 112	-054 112	228 107	135 110		-039 112
l	133 110	222 107	286 103	181 109	090 111	314 101	031 112	166 110	-039 112	

Experimental values obtained by Dice-throwing. 145 dice were thrown 36 times. This is comparable with tests applied to a class of 36 boys.

The lower number in each square is the probable error.

Moreover, it passes equally well when tested by the more stringent C' criterion. This criterion was introduced by Professor Spearman because the ordinary correlation between columns may rise to unity even when the equation

$$\frac{r_{ap}}{r_{bp}} = \frac{r_{aq}}{r_{bq}}$$

is not exactly fulfilled, whereas C' (it is said) only becomes unity when this latter condition is fulfilled. The corrected value is

$$C' = \frac{S\left(r_{xa}r_{xb}\right) - (n-1)\, r_{ab}\, \overline{\sigma_{xa}\sigma_{xb}} - \frac{1}{n} S\left(r_{xa}\right) S\left(r_{xb}\right)}{\sqrt{S\left(r_{xa}^2\right) - (n-1)\, \overline{\sigma_{xa}^2}}\ \sqrt{S\left(r_{xb}^2\right) - (n-1)\, \overline{\sigma_{xb}^2}} - \frac{1}{n} S\left(r_{xa}\right) S\left(r_{xb}\right)},$$

and we obtain

for	*ab* $C' =$	1·01
	ac	1·00
	bc	1·01
	cd	1·03
	Mean	1·01

It may perhaps be urged by someone that this result is only possible because the number of cases, thirty-six, is small. But in the first place this number is as large as in most[1] of the cases in Professor Spearman's list. The only case of very large numbers is that of Bonser[2] and he only applied five tests, and one of his columns failed to reach correctional standard. And in the second place I have already shown theoretically that even with a huge number of cases R uncorrected would be well over 0·9, and that even with one thousand cases there would not yet be the slightest hope of distinguishing between the two hierarchies in Table I.

7. *Conclusion.*

It has here been shown that a certain set of correlation coefficients, which we *know* to contain no General Factor, would be claimed by Professor Spearman as giving further support to the existence of such a factor. *There is therefore nothing to show whether the many cases brought forward by him really contain a General Factor or not.*

It must not be hastily and illogically concluded by anyone that therefore General Ability is a fiction. Its existence or non-existence is, as far as the mathematical argument goes, an entirely open question, which will not be answered mathematically until someone successfully carries out a very much more extensive set of experiments than has yet been attempted. What the work on correlation and hierarchies has shown is as follows:

1. Since correlation does actually exist between tests, there must be either Group Factors or a General Factor present, or both.

2. If there is no General Factor, then it is probable that the Group Factors *overlap* in a complicated fashion; for otherwise there would be no hierarchy. But even this is by no means certain for as a rule

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[1] I gather from Professor Spearman's comments which follow this paper that this is not now the case. It was so in the list here referred to.

[2] See Hart and Spearman, this *Journal*, v. (1912), p. 60 ff.

very few columns reach the correctional standard which Professor Spearman has laid down and these few may be among the minority which do correlate highly even on Thorndike's theory of non-overlapping Group Factors[1].

3. There is not the slightest mathematical evidence so far forthcoming which will enable us to distinguish between *overlapping* Group Factors and a General Factor.

Of course. there may be reasons other than mathematical which may help us to decide; but with these I do not wish to deal in this paper in order not to confuse the issue.

Let me therefore reiterate that all I have shown is that Professor Spearman's calculations are incapable of discriminating between a General Factor and overlapping Group Factors.

Note. I regret to see from Professor Spearman's comments on this paper (see next page) that I have apparently not made the argument perfectly clear. The arrangement of overlapping factors defined by Figure 1 is *of course* not a random arrangement. The point is that it, and numbers of other arrangements which could be made, would be erroneously accepted by Professor Spearman's tests as containing a General Factor, so that if a hierarchy is presented to us we are unable by these tests to say whether it contains a General Factor or some special arrangement of overlapping Group Factors. Professor Spearman's remarks about prearranged cards and coins are irrelevant. Figure 1 is part of the 'definitely stated conditions' and the variation comes in when the dice are thrown.

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[1] Hart and Spearman, *loc. cit.* p. 57.

(*Manuscript received* 1 *March*, 1916.)

SOME COMMENTS ON MR THOMSON'S PAPER.

By C. SPEARMAN.

At the present time, I have little leisure, or indeed inclination, for controversy. But the foregoing clever and interesting paper seems likely to produce grave misunderstanding unless some brief comments are attached.

A relatively minor matter is that the formula which he demonstrates, and upon which his paper is based, was really proved three years ago by myself, being included in the general formula for all correlations of sums[1]. This, however, by no means detracts from the author's originality in applying this formula to the construction of artificial correlations.

More serious is the way in which the author, naturally anxious to make out a strong case, has presented the statistical evidence published on the other side. He describes this as consisting of experiments, in most of which the number of cases did not exceed 36. Whereas it really consisted of twenty investigations, in which the number of cases averaged 87, five times exceeded 100, and had a total of 1753[2]. Such an array of perfectly concordant evidence would be hard to match in psychology, or, indeed, in any other science.

Nevertheless, this fact does not impair the author's main argument. He says that the proof of the general factor will need 'a very much

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[1] Let $a_1 \ldots a_l$ denote the dice common to the two series of throws, while $b_1 \ldots b_m$ and $c_1 \ldots c_k$ are those peculiar to each series. The author has proved that

$$r_{(a_1+\ldots+a_l+b_1+\ldots+b_m)(a_1+\ldots+a_l+c_1+\ldots+c_k)} = \frac{l}{\sqrt{l+m}\,\sqrt{l+k}}.$$

But the value of the term on the left of this equation is by my general formula (this *Journal*, v. p. 419):

$$\frac{l\,(r=1)+S\,(r=0)}{\sqrt{l+m+2S\,(r=0)}\,\sqrt{l+k+2S\,(r=0)}},$$

which thus gives at once without any calculation the result required.

[2] *The Eugenics Review*, 1914, Oct., p. 8.

more extensive set of experiments.' I will go farther, and maintain that *no* set of experiments, not a million cases with the purest general factor, could produce a hierarchy distinguishable from one of the sort shown in the foregoing paper; moreover, it makes no difference whether this hierarchy is constructed by formula or by actually throwing dice.

This brings us to the real weakness in the paper, one which, frankly, I should not have expected from the author. Correlation belongs to the mathematics of probability, and these invariably consist of two elements: firstly, there are certain definitely stated conditions; and secondly, there is in all other respects complete variation, if the treatment is theoretical; or very frequent chance variation, if the treatment is only empirical. To one thing no admittance is ever granted, save by way of mere illustration; this is *undefined, arbitrary arrangement.* We may search through all the theorems of Bernouilli, Poisson, Bayes, Tschebycheff, or Laplace; we may go from one end to the other of Yule's admirable little 'Theory of Statistics'; not once is any such thing brought forward, either in proof or in disproof. But quite undefined and arbitrary is the distribution of the 36 group factors in the foregoing paper (Fig. 1), upon which the whole edifice of correlations has been erected. One might as well attempt to demonstrate Moivre's picquet theorem with prearranged packs of cards, or to test any of the probabilities of coin-tossing with sequences of heads and tails devised at will.

It may be urged that the plan of distribution is evident on inspection, and presents a typical case of randomly 'overlapping' factors. Nothing could be more remote from the truth. The effect of a really random overlapping of all the factors has already been published by myself; under this condition, the correlation between any two columns is equal to that between the two tests heading the two respective columns[1]. The present distribution is, on the contrary, an extremely artificial one. The author himself admits it to have cost him as much time and thought as he could spare. But he cannot have reached an insight into the underlying principle, or he would never have proposed to remedy matters by more extensive experiments. And yet no one, until he does learn the principle, can possibly tell whether the distribution conforms to any conceivable psychological theory, or has any bearings on psychology whatever.

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[1] 'The Theory of Two Factors,' *Psych. Review,* XXI. March 1914, p. 109. The proof was not given in that paper, however, and its publication has since been delayed by an urgent call in another direction.

This absence of any stated principle in the distribution of factors is quite sufficient to invalidate all the conclusions of the foregoing paper. I may, however, mention that I propose in a subsequent paper to demonstrate the nature of the principle really involved. It happens to have long been known to me, being an integral part of the proof of the above mentioned effect of random overlapping. On this principle being established, the author's method will at length be able to bear the good fruit which its ingeniousness deserves. It will be found to have the same curious issue as the other evidences brought against the 'theory of two factors'; for it will turn out to be really a witness in favour.

ADDENDUM.

With reference to the concluding note of Mr Thomson, I must reiterate that correlations belong to the mathematics of probability, in which undefined 'special arrangements' have no place. It is, doubtless, *possible*, that out of the different arrangements permitted by the author's figure 1, namely, 2,344,700,000,000,000,000,000,000, 000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000, 000,000,000,000,000 trillion trillions, his 'special' one[1] *may* have turned up in all the 20 investigations concerned by mere accident. It is also possible, that perfect hierarchies will continue to occur in every future investigation equally by chance. But with such possibilities we need not reckon.

In the present case, there are further reasons against his 'special arrangement' of unknown meaning. For all that can be seen, it might involve psychological absurdities. It might even re-introduce the General Factor by a back door. And in point of fact, as my next paper will show, it does both!

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[1] He has only proved the existence of a single one.

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THE RELATIONS BETWEEN MAGIC AND ANIMISM.

By CARVETH READ.

- *I. The Question of Priority:*
    - *Wundt's Theory.*
- *II. Magic and Religion:*
    - *Frazer's Theory.*
- *III. Ideas and Practices of Magic adopted by Animism:*
    - (*a*) *Invisible Force.*
    - (*b*) *Fetich.*
    - (*c*) *Omens.*
    - (*d*) *Prayers.*
    - (*e*) *Religious Rites.*
- *IV. Retrogradation:*
    - (*a*) *Amulets.*
    - (*b*) *Omens.*
    - (*c*) *Prayers.*
    - (*d*) *Ritual.*
    - (*e*) *Magic remains when Religion changes.*
- *V. Spirits know Magic, teach it, and inspire Magicians:*
    - *Black Magic.*
- *VI. Spirits operate by Magic:*
    - (*a*) *By Possession.*
    - (*b*) *By direct Action, Metamorphosis.*
    - (*c*) *By Charms and Spells.*
- *VII. Spirits are controlled by Magic:*
    - *Shamanism, Egyptian Ritual, Legal Regulation, Fate.*

I. THE QUESTION OF PRIORITY.

MAGIC and Animism now everywhere flourish side by side, or in confused union, and, by those who believe in them, are not discriminated as they may be by a spectator. As to their origin, they are both prehistoric; it is useless to inquire about it amongst believers, or to look for any sort of direct proof. In a former article[1] I offered some general considerations in favour of the priority of Magic; one of which was that, among the most backward peoples, Magic is further developed than Animism; but a re-examination of the reports upon such peoples has made me doubt whether this is true. On the other hand, I then said nothing of the opposite opinion, that Animism is prior and Magic derivative: an opinion held by many and, amongst them, by Professor Wundt, whose treatment of the problem claims attention. He holds that the idea of the soul has three principal sources: (1) the gebundene Seele, or Kórperseele (consciousness as an attribute of the body), is immediately given, without the need of any reflection, as a result of perception-associations; for thinking, feeling and willing are constant elements of a living body[2]. The influence of this idea is seen in the practices of making offerings to the dead at their tombs, of preserving the body itself, of treating the blood and various parts of the body as vehicles of the soul, the use of hair and nail-parings in sorcery, and so forth. By contact, this soul can be transfused into other things. But the freie Seele, a being differing from and opposed to the body, is suggested (2) by the cessation of the body's living functions with the last breath– the Hauchseele; and also (3) by dreams and visions—the Schattenseele. This third conception gradually subordinates the other two, and has the chief part in the development of Animism and Mythology[3]. As to sorcery (Zauber), it is, at first, always attributed to the human will; in as much as this is the original type of causation. Ordinary events raise no question of causes for the Naturmensch; but only extraordinary occurrences do so, such as sickness and death. Even pain or death from wounds is a matter of course, for the antecedents are visible to him; but pain or death from sickness has no such customary antecedent; so to explain them he imagines an enemy who can operate at a distance by sorcery. Sorcery he conceives of as an operation of one soul upon another; either directly, or indirectly by various appliances, such as pantomimic injury by means of an image. Pantomime is at first believed to affect the victim's soul, and so to cause sickness in his body; but the oftener such rites are repeated, the more the intervention of the soul is obscured. For in many linked associations, especially where the first and last links stand as means and end, the middle links are apt to disappear; and since these are, in this case, ideas about the soul, there remains, after their loss, the indefinite idea of some incomprehensible action at a distance by means of the pantomime: it is then no longer Sorcery but Magic. Similarly, a fetich, or a talisman or amulet (which differs from a fetich only in not being the object of a cult), originally owes its power to an indwelling spirit, but may degenerate into a magical object[1]. Magic, therefore, is always derivative and secondary; and Animism is entirely independent of Magic.

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[1] This *Journal*, VII. 169–74.

[2] *Mythus und Religion*, 78, 9.

[3] *Op. cit.* 125 *et seq.*

This theory is worked out with Professor Wundt's usual comprehensiveness and methodical clearness; and the exposition abounds with interesting discussions; but it has not convinced me. The Körperseele, as an attribute of the body, is, surely, not a soul at all. Customary perception of other men interpreted by self-consciousness, with the habitual treatment of others (and of ourselves by others) as conscious bodies—making it difficult to conceive that a corpse is really dead—no doubt influences animistic rites; for even though the soul seen in dreams may be believed to live, having the consciousness of its former body in dreamland, yet some consciousness seems to remain with the body in the grave. Many rites performed at a tomb, however, may also be understood in relation to a belief that the soul, though having a separate existence as seen in dreams, still desires to reinhabit its body, or to protect its buried treasures, and therefore, though its new home be far away, frequently returns and haunts the neighbourhood of its tomb. But it is not, I take it, until the soul seen in dreams has become an object of popular belief, that any idea can be formed of a body-soul—or more properly of a soul within the body, and thence of a soul-stuff of the body—which leaves the body at death (and under other conditions) as the vehicle of consciousness. This soul-stuff which leaves the body at death may easily come to be identified with the breath, but not until the discussion of dreams has given rise to the belief in a separable soul. The fact that in cold weather the last breath (or any other!) may appear for a moment as a vapour, and is never seen again, cannot by itself suggest a separate persistent existence like that of the soul; and over a considerable part of the earth such a vapour is seldom, or never, formed by the breath. The Körperseele and the Hauchseele, therefore, are not independent sources of Animism, but are entirely dependent for their imaginary existence upon the Schattenseele, upon the growth of a belief in a separable soul as seen in dreams.

As to Sorcery, it may be defined as Magic practised with the aid of spirits; and since its existence implies that a belief in spirits and their influence has already formed itself, it may also be believed to operate, in the first place, on the souls of its victims and so, in the second place, on their bodies. Then, as Professor Wundt explains, a process of retrogradation sometimes occurs, in the course of which the spirits are forgotten, and only the mechanical rites remain as a residuum of bare Magic. Similarly, a fetich sometimes becomes a merely magical talisman or amulet. This is hardly disputable; but it does not prove that the degeneration of Sorcery is the only source of Magic, or that Magic has not (for the most part, indeed,) another, independent origin. The issue is difficult to argue upon the ground of facts, because magical practices are of such high antiquity. If, for example, one should urge that the intichiuma ceremonies of the Arunta are not, so far as we have evidence, designed to operate by spiritual power upon the souls of the emu or the witchettygrub, but directly to promote by Magic the fertility of these objects, it might be replied that such, indeed, may be their present character, but that the original intention must have been to promote fertility by first influencing their souls, and that this has been forgotten. Or, again, if one should point to the little stones—tied up in bark and believed by the Kaitish to be stores of evil Magic—as having no mark of the fetich, no character to indicate that their power is due to spirits, so that they seem to be merely magical, the answer would be ready, that by long use and retrogradation they may have ceased to be fetiches, but that a good theory requires them to have been of that nature aforetime. Thus any case of apparently bare Magic may be treated as a residuum of lapsed Animism; or, should its origin be recent and ascertainable, it may still be said to have been constituted by analogy with such residua.

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[1] *Op. cit.* 262 *et seq.*

We are driven, therefore, to rest the argument upon the psychological conditions of such beliefs. Is the nature of the human mind, so far as we can interpret it at the savage level, such that the belief in Animism necessarily precedes and (later) gives rise to the belief in Magic; or is it possible to indicate conditions that may independently give rise to Magic? According to Professor Wundt, as I have said, Sorcery precedes Magic and, at first, is always attributed to human volition, because this is the original type of causation. Contrary to Hume's doctrine (he says) the ordinary course of events does not excite in the savage the idea of causation, or the need of explanation. Customary series of events belong to those matter-of-course properties of things which he, eben wegen ihrer Regelmässigkeit, unmöglich hinweg denken kann[1]. It is the unusual occurrences—accidents, storms, rain (where much desired) and especially sickness and death—that awaken in him the need of causal explanation. He is accustomed to pain from wounds, where he sees the conditions on which they always follow; but the pain of disease has no such antecedents, and he supplies the gap in routine by free associations, imagining that this pain also must be the work of some enemy. For in the regular course of events there is for him only one region in which an effect appears notwendig verknüpft mit dem Vorausgehende: namely, that of his own voluntary actions. The connection is, indeed, only a matter of fact; but it includes the sensations and feelings of his own power über den Eintritt des Ereignisses. This, as Berkeley saw (says Professor Wundt), is the true origin of the notion of causality; though the true principle of causality requires the elimination of this subjective ground of its origin[2].

It is true, of course, that the savage has no definite idea of the principle of causation; but he has obscure ideas of all its chief marks—the need of some antecedent for every event, regularity of connection, and proportionality[3]; and probably, in the depths of his mind, the abstract principle has made some progress toward maturity[4]. (*a*) The ground or source of such ideas, according to Hume, is customary experience; and that such experience includes its own causation (and, therefore, needs no explanation) is proved by Professor Wundt's contention that it is the unusual which first demands causal explanation; because there the familiar causation is missing; so that the savage tries to fill up the lacuna, as best he can, according to the type of what is usual. But (*b*), according to Professor Wundt, there is in the regular course of events only one region in which the idea of causation (though illusory) first arises: namely, our own actions, in which we are aware of our own power over the beginning of the event. And no one, I suppose, doubts that the notion of power is derived originally from the consciousness of our own exertions: read, by sympathy, into the actions of other men and animals and, by empathy, into the movements of trees, stones, winds and waters. All this, however, occurs so early in the individual and in the race (probably in the higher animals) that, before the need of causal explanation is felt, the world is seen as if pervaded by forces, which are manifested in *every* usual course of events and not merely in voluntary actions. Again (*c*) power is only one character of the primitive belief in causation: another, not less important, is uniformity;

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[1] *Op. cit.* 263.

[2] *Op. cit.* 267.

[3] This *Journal*, VII. 177.

[4] This *Journal*, VIII. 15.

and the study of our own actions is notoriously unfavourable for the discovery of uniformity. Without any obvious reason for it, our visceral activities can hardly be controlled at all; our habitual actions, once started, go on of themselves, and often begin without (or contrary to) our wishes, especially gestures and expressions; in fatigue control flags, in disease is often lost[1]; we do not always give the same weight to the same motives, nor fulfil our intentions whether good or bad. But if the relation of will to action is not apparently uniform, it cannot be seen to be necessary; so that this is generally regarded as the peculiar region of caprice. But, further (*d*), were the connection between volition and movement more constant than it is, it would still be most improbable that ideas of causation should be chiefly drawn from our consciousness of it; for the interest of action lies not in the mere control of our own movements, or power over the beginning of events, but in the attainment of our ends; and there is no department of nature in which the failure of connection is nearly so impressive. It is because of this failure that the savage becomes fascinated by ideas of magical and (later) of spiritual aid. Finally (*e*), no control is exercised by the will over pain—headache, colic, rheumatism, etc.; yet we are told that the savage, when so afflicted, refers his sufferings at once to the will of some enemy operating at a distance. Such inferences are not primitive, but the result of a long growth of superstitions. Among Australian aborigines, disease and natural death are generally believed to be caused by the magical practices of an enemy, not merely by his will.

We are not, then, obliged to infer that, because volition is the type of necessary connection, Sorcery, or any other form of Animism, preceded Magic. On the other hand, there are conditions that may have given rise independently to a belief in Magic. The savage has frequent experience of regular trains of event which, for want of analytic ability, he does not clearly understand, but which exist in his mind as types determining his apprehension of other sequences. When two interesting events happen about the same time, the later recalls the earlier; because the impression of the earlier, having been deep, perseverates, and is apt to be re-excited by almost any occurrence. An association is then formed between them, and obtains as strong a hold upon the mind as less interesting ones can by many repetitions. The man judges them to be connected; and expects the coincidence to repeat itself as usual occurrences do; and the more vividly the more he desires or fears it. Such expectations, together with the idea of invisible force and the oppression of mystery, by degrees establish the belief in Magic. Probably no traveller amongst wild peoples, or observer of the unsophisticated at home, will think that too much stress is here laid upon the power of coincidence to create general expectations. The Egyptians, says Herodotus, "whenever a prodigy takes place, watch and record the result; then, if anything similar ever happens again, they expect the same consequences[1]." They had merely reduced to a system the universal practice of unanalytic minds.

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[1] Hume, *Inquiry*, § vii.

II. MAGIC AND RELIGION.

That Magic probably precedes Animism in the history of human philosophy was formerly argued by me[2] under the erroneous impression that it was also the opinion of Sir J. G. Frazer. His position, however, as stated in the *History of the Kingship* and in the *Magic Art*[3], is that Magic, as a means of gratifying one's desires, is prior to Religion (not to Animism in general), conceived of as a means of attaining one's ends by the propitiation of spirits. This is a much narrower contention, and proportionally more defensible. Whilst the priority of Magic to Animism seems to me to have some low degree of probability, the priority of Magic to Religion, as the propitiation of spirits, seems probable in a much higher degree; since we have plain information that the Australians practise Magic extensively and also believe in ghosts without propitiating them[4].

On the other hand, Sir J. G. Frazer's explanation of how Religion superseded Magic is questionable. He conjectures "that a tardy recognition of the inherent falsehood and barrenness of magic set the more thoughtful part of mankind to cast about for a truer theory of nature and a more fruitful method of turning her resources to account. The shrewder intelligences must in time have come to perceive that magical ceremonies and incantations did not really effect the results which they were designed to produce," and the wizard inferred that, "if the great world went on its way without the help of him or his fellows, it must surely be because there were other beings, like himself, but far stronger, who, unseen themselves, directed its course[1]." To these he addressed himself, and sought by prayer what he had formerly hoped to obtain by Magic. Such is Sir J. G. Frazer's suggestion, offered tentatively, and (surely) not agreeing well with the facts which he has set before us. For he has shown that no amount of experience can discredit Magic, generally, in untutored minds; that certain kinds of Magic are sometimes pushed into the background by Religion, but never forgotten; whilst other kinds of Magic become fused with Religion itself and constitute an important part of its rites; so that they are indeed few who can be said to have betaken themselves to religion "instead of magic." Besides, the only ground upon which a penetrating mind, that had discarded Magic as discredited by experience, could resort by preference to the worship of spirits must be that experience showed prayer and sacrifice to be more efficacious than Magic in attaining our ends. Is there reason to think that (lucky coincidences apart) this has ever happened? Must we not rather say that, whether one relies on Magic or on Religion, experience of failure counts for almost nothing? So many excuses are at hand.

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[1] B. II. 82; Rawlinson's Translation.

[2] This *Journal*, VII. 167.

[3] Vol. I. c. 4.

[4] But see the footnote at p. 235 of *The Magic Art*, I.—"faith in magic is probably older than the belief in spirits." In the same note, a passage in Hegel's *Philosophy of Religion* is referred to as anticipating the doctrine of the priority of Magic to Religion. The passage, as translated in an appendix (p. 423–6), shows, however, no conception of Magic as akin to natural law, as it is described in several passages of *The Golden Bough*, but treats it as a belief in any human being "as the ruling power over nature in virtue of his own will." This is rather an anticipation of Professor Wundt's doctrine concerning Sorcery; which Hegel seems not to have distinguished from Magic.

The matter presents itself to me in this way: at first, belief in Magic arises as a means of obtaining good and averting evil. Grounded, as Sir E. B. Tylor says[2], in the desire "to discover, to foretell, and to cause events," it is irresistibly attractive by its power of increasing one's confidence, of making sure.

Secondly, at some stage after the rise of Animism, religious practices are added to the magical, to make assurance doubly sure, just as one magical practice may be added to another, a rite to a spell. At this stage, there is no sense of opposition between Magic and Religion. That, in fact, they are opposed in their nature, as an invariable to a capricious force, even if this difference were appreciated by savages, need not prevent their co-operation; for Magic is known to be a tendency that may fail of the effect desired, either by the counteraction of superior Magic, or by imperfection in the rites; and one sees no reason why a spirit should not be supplicated to supplement the imperfect rites, or to frustrate the superior Magic. To supplicate the intervention of spirits, once they are fully believed in, is an act so simple and natural, that we may wonder how it should ever be omitted where Animism prevails.

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[1] *Op. cit.* pp. 237–9.

[2] *Primitive Culture*, I. 116.

For what can be more spontaneous than to ask the aid of one's father or friend, and why not ask the spirits disembodied as freely as those in the flesh? Unless, indeed, it is believed that the less one has to do with them the better! Probably, this consideration does restrain the early impulses to pray.

Thirdly, certain forms of Magic come, after a time, to be discountenanced or punished: black Magic, because it is anti-social and criminal; other forms of Magic, when carried on by private practitioners, because they infringe the monopoly of supernatural power that has now been claimed by dynasties and priesthoods; or because (in other words) the public gods are jealous of all competitors. Legitimate Magic has now been incorporated with Religion. And the power of Religion becomes greater than that of Magic without Religion, not only by the support of the influential classes, but also because Religion, whether as worship of the public gods or as sorcery or devil-worship, afflicts the human mind with peculiar terrors; and, again, because Religion, should it clarify morally and aesthetically, appeals more and more to the affections—to the family affections and to loyalty. The impersonality of pure Magic sets it (as it does Science) at a great disadvantage in this competition.

Finally, whilst the failures of Magic always need to be excused—as by a mistake in the rites or by the opposition of stronger Magic,—Religion brings with it a new excuse for failure, namely, the caprice of the spirits or gods propitiated. At their pleasure they may reject the prayers and sacrifices. Persistence in such conduct on their part is sometimes met by banishment, deprivation of rank, or other punishment—the civilized methods of China; at other times by praying louder and sacrificing more extravagantly, in the style that culminated in Mexico together with the power of barbaric priesthood. Still the gods may be obdurate; and, probably, to excuse the failure of propitiation by the caprice of the gods was, from the first, looked upon as a clever device: not observing that the caprice of the gods was incompatible with the security of their worshippers; and, therefore, in conflict with that desire of security which is the root of the whole supernatural structure, whether magical or religious. This conflict must have consequences.

Religion, then, very probably, is of later growth than Magic; but whether Animism, as a belief in separable (or separate) spirits, human or other, is later or not than Magic, there is insufficient evidence. At any rate, their origins are independent. Perhaps my own preference for the priority of Magic depends, partly at least, on the convenience of that view in arranging the following considerations.

III. IDEAS AND PRACTICES OF MAGIC ADOPTED BY ANIMISM.

(*a*) Fundamental in Magic, as now practised, is the idea of force, invisible and intangible, which can operate at a distance without any visible or tangible vehicle. The idea may have been formed by analogy with several natural phenomena, such as the wind, radiant heat, sound, odour[1], and it is involved in a savage's beliefs concerning the efficacy of charms, rites and spells. When a man dies, he lies speechless and motionless, no longer exerts his accustomed force in any way; but, if seen in a dream, he still speaks and acts, perhaps wrestles with the dreamer. Here, then, is the force that had deserted the body[2]: it is visible and tangible in dreams only, or perhaps sometimes by twilight; or to gifted seers, or to dogs or pigs. The force exerted by the ghost or spirit is the same thing as force magical, except in one character: its action is capricious, depending on the good or ill will of the ghost or spirit; whereas purely magical forces have uniform tendencies. Magic, then, prepares the idea without which the appearance of a dead man in a dream, after his body had been buried or burnt, would have no reality or practical consequences for the living. Comparison with shadows and reflections could not lend reality to dreams; for they require the presence of the body, and themselves have no mechanical significance. Their association with the spirit probably follows the use of their names to describe the spirit, which cannot at first have a name of its own. The same names being used for shadow or reflection and for spirit or dream-soul, the things are in some measure identified; and then the idea of force may be associated with shadows and reflections; so that the falling of a shadow upon a man may injure or slay him.

Magic-force and spirit-force being the same thing, the question whether *mana* is a magical or animistic notion is misleading. It will be conceived of by different tribes in one way or the other, according to the relative prevalence in one or the other tribe of the animistic or of the magical mode of explanation. Not that we can reasonably consider *mana* as the original common ground from which Magic and Animism have been differentiated. The origin of these systems, or congeries of ideas and practices, must rather be sought in a multitude of particular beliefs, from which *mana* is a generalization.

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[1] This *Journal*, VII. 175.

[2] This *Journal*, VIII 78.

(*b*) As formerly observed[1], it is reasonable to expect that, as the ghost-theory spread, the magical force of things should be conceived of as spiritual; so that amulets and talismans would come to be regarded as owing their virtue either to a controlling spirit, or to an indwelling spirit peculiar to each: in the latter case the charm is a fetich. When this happens, the efficacy of such objects is no longer expected to be uniform, but depends on the mood of the indwelling or controlling spirit. The fact that their efficacy, though formerly presumed to be uniform, never was so, favours the new interpretation; and this having been accepted, a cult (or a discipline) of the spirit is apt to follow. Thus the magician becomes a sorcerer or a priest.

In North Central Australia, short sticks or bones are used for pointing at an enemy and directing magical force against him. Only the Guangi and other tribes of the Gulf coast manufacture dead-men's bones (femur or fibula) into pointers; but these are traded southward, and are considered more potent than other pointers[2]. A stick, then, is the primitive talisman, often 'sung' with a spell in Alcheringa words, which the operator himself does not understand; and it acts by pure magic. The dead man's bone is more potent, perhaps only because it is more oppressively gruesome and terrifying; we are not told that it carries the power of its former owner's ghost; but how near the thought must be. In South-east Australia, pointing with the bone (human fibula) is very common; in pointing you name your victim and say how he is to die; but that the efficacy of the rite does not depend upon a spirit is shown by this, that, when pointing, you tie a cord of human hair (attached to the bone) tightly around your upper arm, in order to drive blood into the bone. In other rites, however, in which the fat of a dead man is used, the ghost of the dead is believed to assist the operation; for a man's fat, especially kidney-fat, is the seat of his prowess and other virtues[3]. An easy extension of ideas by analogy would interpret a rite in which a dead man's bone is used, and which is on that account more potent, as owing its superior potency to the assistance of the dead man's ghost. It seems easy; but resistance to the progress of explanation is not peculiar to the civilised mind.

Again, in South-east Australia, a *bulk*—a pebble, usually black and roundish—is carried by wizards as a powerful talisman. A native dreamt of seeing two ghosts by his camp fire, and, on waking, found a *bulk* where they had stood[1]. How could such an impressive experience fail to raise a belief in a connection between ghosts and *bulks*: both so attractive to the imagination, and alike mysterious and powerful? Many such situations must strongly indicate an extension of the ghost-theory, once it has been formed, to explain the influence of charms and rites. A time comes with some tribes when the activity and ubiquity of spirits is so much a matter of course that every mysterious power is apt to be ascribed to their presence. If a person or thing was originally taboo, either by inherent virtue or by force of a spell or curse —a talisman dangerous to everyone who violated its sanctity—Animism explains the danger by the wrath of a protecting spirit. A boundary having long been taboo, a spirit is imagined to protect the boundary, and becomes the god Terminus. Diseases, at first attributed to Magic, are later explained by Animism; so that whilst an Australian wizard is content to suck a magically-implanted stone or splinter from his patient's body, a priest of the Dyaks, having sucked out a similar object, calls it a spirit[2]. The wonder is that at this stage of thought any purely magical power can survive.

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[1] This *Journal*, VIII. 6.

[2] Spencer and Gillen: *Northern Tribes of Central Australia*, 453, 463.

[3] A. W. Howitt, *Native Tribes of South East Australia*, 359–361–367.

(*c*) An Omen is regarded as giving warning of some event, although between event and omen there is no traceable connection—in this resembling many magical operations; and at first omens may have been always so conceived of, and only by degrees distinguished from charms and spells. Like magical things or actions, they may sometimes have been adopted on the ground of coincidence—as if, after an unsuccessful expedition, one should remember having heard a woodpecker just before setting out; sometimes on the ground of analogy—as if an animal seen on the left hand should suggest weakness and clumsiness and, therefore, failure; or even by arbitrary determination,—as perhaps in the distich about magpies:

One for a wedding, two for a birth,
Three for a funeral, four for a dearth,—

which may depend on nothing further than a superstitious attitude toward magpies, a survey of the chief concerns of human life, and a proclivity to versification. But in most parts of the world omens have come to be treated as divine premonitions; and (as I have said)[3] the marks which distinguish omens from the rest of Magic are such as to favour a growth of the belief that they are sent by spirits. For (i) omens are generally of such a nature as not to suggest any direct causation of the event—*e.g.* the note of a bird or insect, a shiver or stumble, the appearance of a hare—so that the operation of some invisible force of unknown origin is implied. (ii) They are often remote in time from the event, and in this resemble warnings given by someone who knows or controls the future. (iii) They are not indications of events in detail, but only of the kind of event as good or evil; and in this, too, they resemble warnings. (iv) They are not, like charms, rites and spells, the property of a man or acts of a man; and must, therefore, if warnings (implying foreknowledge), proceed from some spirit. It would be absurd, however, to suppose that the nature of omens as divine premonitions was originally reasoned out in this way. We must look for the beginning of the idea in some direct stimulus to the animistic imagination; and this may well have been given in dreams. For, first, dreams are often themselves regarded as omens; and, secondly, they are immediate manifestations of the spirit-world. An impressive event coinciding with the remembrance of an impressive dream makes the dream an omen, and may make any prominent feature of the dream into a warning or encouragement given by a spirit whenever the like of it recurs in future. From such a starting-point, the animistic interpretation spreads to other omens, but with the usual irregularity of savage theories.

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[1] Howitt, *op. cit.* 378.

[2] E. B Tylor, *Primitive Culture*, II. 148.

[3] This *Journal*, VII. 183.

(*d*) Spells addressed to any object tend to the personification of it; as the ghost-theory gains strength, the personified object may acquire an indwelling or controlling spirit, and the spell addressed to it may become a prayer. Not that this is the only way in which prayer may originate; for (as remarked above) nothing can be simpler or less in need of explanation than the invoking of the spirit of one's relatives (the ghost-theory having been established) to help one or, at least, not to persecute. Indeed, it is not unreasonable to suppose that this was often attempted, and not persisted in for want of obtaining an answer; so that a long tentative age preceded the settled custom of prayer. Nor is it easy to see how belief in the efficacy of prayer (beginning in this way) could ever have been established, unless it were confirmed by coincidence—just like Magic. However, the earliest form of prayer and of spell (whichever may have been the earlier) is the same—a simple expression of desire—whence prayer and spell have been differentiated. Hence it may be impossible to decide whether a given ejaculation belongs to one class or to the other. Thus Mr R. W. Wilkinson tells us that, amongst the Mafulu of New Guinea, when fishing in the river Aduala, the fishers, after forming a weir, but before fixing their net, all join in a sort of prayer or invocation to the river: "Aduala, give us plenty of fish that we may eat well[1]." But he expressly says that, whilst they believe certain parts of the river, such as a waterfall or deep pool, to be haunted by spirits, they do not believe this of the river itself[2], and that generally their Animism is very backward. The ejaculation, therefore, seems to be a spell. Compare with it the Jakun spell to bring monkeys within shooting distance:

> Come ye down with souls enchanted,
> Monkeys, by my spells enchanted[3].

If, then, the original form of prayer and of spell is often the same, the sole difference between them lies in the intention of the speaker. One of the Kurnai, to stop the gales, cried: "Let the West Wind be bound[4]," and this is evidently a command and a spell; but if he regarded the wind as controlled by a spirit, a change of tone would make it a prayer. Still, whether with the spread of Animism a spell shall become a prayer, must depend upon whether the spirit addressed is believed to be the more easily importuned or coerced.

The tabu that often attaches to the names of the dead and of other spirits may easily have been derived from the magical practice of summoning by name or of naming the victim of a rite. To call a living man by name draws his attention and often brings him to the spot; a magical naming is (from the temper of Magic) uniformly effective; so that to avoid such control names are kept secret; and when ghosts are believed in, naming has the same power over them and is, therefore, extremely dangerous. Hence, in Sorcery (a dangerous art), to introduce the names of spirits into spells is to secure their presence and assistance: and, in prayer, to use the true name of the spirit or god addressed may be indispensable; the worshipper's intention is not enough.

(*e*) With the spread of Animism, magical rites often become religious. This may occur by simply adding the invocation of a spirit to a magical rite (as a spell may be added) in order to strengthen it—the two actions remaining quite distinct; or some degree of fusion may take place, obscuring more or less the original character of the practice. The Kai (Papuans of northern New Guinea) make rain by muttering a spell over a stone, and at the same time calling upon Balong and Batu to drive away Yondimi, a woman who holds up the rain; and when rain enough has fallen, they strew hot ashes on the stone, or put it in the fire, to stop the rain[1]. The animistic invocation, being omitted from the process of stopping the rain, seems to be merely adscititious to the making of it. Again, "when rain is badly wanted in the Oraon country, the Oraons of each village fix a day for the rain-making ceremony. On the morning of the appointed day, the women of the village, with the wife of the village-priest or Pahan at their head, proceed to the village spring or tank, and there, after ablution, each woman fills her pitcher with water, and all proceed in a body to a sacred pipar-tree.... On their arrival at the sacred tree, all the women simultaneously pour the water in their pitchers over the root of the tree, saying 'May rain fall on the earth like this.' The wife of the village-priest now puts marks of vermilion, diluted in oil, on the trunk of the tree. After this the women depart, and the Pahan or village-priest proceeds to sacrifice a red cock to the god Baranda at the spot....In this case, apparently, by direct alliance, sacrifice and the anointing of the tree with vermilion have been superimposed upon what was once, perhaps, purely a ceremony of imitative magic[2]." Mr Warde Fowler tells us that an ancient Iguvian document contains instructions for the lustration of the people before a campaign: the male population assembled in its military divisions; around the host a procession went three times; at the end of each circuit there was prayer to Mars and to two female associates of his power, to bless the people of Iguvium and to curse their enemies: and he observes that religion has here been imposed upon the original magic-ceremony. For the idea must have been that, by drawing a magic circle around the host, it would be protected in the enemy's country against hostile magic by being rendered holy. "A later and animistic age would think of them (the soldiers) as needing protection against hostile spirits, of whose ways and freaks they were of course entirely ignorant." Hence the prayer to Mars[3].

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[1] *The Mafulu*, 193.

[2] *Op. cit.* 272

[3] Extract from a spell in Skeat's *Malay Magic*, 571.

[4] Howitt, *op. cit.* 397.

Similarly, rites connected with seed-time and harvest, originally magical, become religious, as beliefs grow up in spirits of the rice, or corn, or vine, or in gods of agriculture or fertility. Thus, as magical power is the same thing as spiritual power, magical practices may be not merely the antecedents but even the foundations of religious practices. Long after the development of Animism, magical practices are maintained by natural conservatism; if priests exist, they try, of course, to annex such practices to the worship of their god; and if the annexation is accomplished, whether by priestly management or by a popular movement, no incongruity may be felt for a long time between the uniformity of Magic and the caprice of Animism; the whole celebration is called Religion, and becomes suffused with religious feeling.

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[1] J. G. Frazer, *The Belief in Immortality*, 288. For similar instances see the same work, pp. 335 and 375.

[2] Sarat Chandra Roy, "Magic and Witchcraft on the Chota Nagpur Plateau," *J. R. A. I.* 44, p. 330. I have slightly altered the last sentence, which seems to have been misprinted.

[3] *Religious Experience of the Roman People*, 215.

IV. RETROGRADATION.

On the other hand, in all these cases, the animistic interpretation of the power of fetiches, omens, prayers, rites, whether original (as Professor Wundt holds) or acquired, may be lost, and a magical interpretation alone remain. For one's mind becomes so engrossed with objects or practices (such as charms or prayers) that are regarded as necessary to the gratification of any masterful desire, that not only irrelevant ideas, but any ideas not indispensable to the connection between the objects or practices and the gratifications, may be forgotten; and as objects or practices acquire interest in themselves, even the gratifications formerly desired may be forgotten. Just as such means as money or books, business or study, may become ends to the exclusion of further enjoyments, so images or rites, at first subsidiary to the obtaining of demonic aid in love or revenge, may be cared for with a fervour that excludes the thought of any intervening means to those ends (especially such means as a capricious spirit who may fail one), and may even be practised under a vague fear or discomfort in the omission of them, when no particular purpose is any longer remembered. On the principle of least effort, we attend only to what is necessary.

(*a*) A saint's finger-joint may at first be treasured as a fetich having the power of the saint to save from shipwreck; after a time it may be carried as an amulet without any thought of the saint's interposition; whilst the evil to be averted is more and more vaguely imagined. Seeing that spiritual and magical agencies are the same invisible, unintelligible force, how easy to interchange them.

(*b*) Similarly omens, from being divine messages, each relating to a particular undertaking, may come to be merely occurrences that encourage or discourage a man, or a tribe, at any time; because, by tradition, they are lucky or unlucky. Or practically the same result may be reached by philosophy: as with the Stoics, who explained that omens are prophetic not as sent by the gods, but as involved in the same procession of fatal events. Fate, before any laws of nature had been discovered, was nothing but all-comprehensive Magic: which left out or mediatised the gods, because, in a philosophical consideration of the world, they are worse than useless.

(c) As to prayers, in any rational conception of them, the form of words conveying them cannot matter to a god, as long as they are piously meant and devoutly meditated. Yet everywhere there has been a tendency to reduce them to strict formulae, any departure from which may, it is feared, impair their efficacy. So far as this occurs, their operation is magical; they have become spells. Such is the result of custom, with mental inertia too dull to think; of an irreligious temperament, getting quickly through an uncongenial task; of a superstitious unimaginative spirit, afraid to omit any traditionary means of safety and for whom a praying-wheel is the way of peace. To rob prayer of its religious meaning, there is the ever-present example of the magical spells that operate by their own force. A form of words, whether magical or supplicatory, that has been among the antecedents of a time of peace or of gain, seems to be amongst its causes, and is repeated that such a time may continue. Of the countless cases in which prayers have degenerated into spells, none is more instructive than the one recorded by Dr Rivers in his account of the dairy ritual of the Todas. The prayers offered during this ritual are uttered in the throat, so that the words are undistinguishable; and they are divided into two parts: first, a list of sacred beings and objects mentioned by sacred names, much of it unintelligible; and, secondly, a petition for the protection and welfare of the buffaloes: the former is now the more important; the latter is apt to be slurred over, or perhaps omitted[1]. Of the Roman public prayers, Mr Warde Fowler says: "The idea that the spoken formula (derived from an age of Magic) was efficient only if no slip were made, seems to have gained in strength instead of diminishing, as we might have expected it to do with advancing civilization[2]." To justify the belief in formulae it may be asserted that the gods themselves prescribed them: an excuse for the superstitious dread of altering what is traditionary, and for the persuasion that the form itself has mysterious virtue.

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[1] *The Todas*, 213, 30.

[2] *Religious Experience of the Roman People*, 286. But whether we should expect the idea to weaken with advancing civilization must depend upon whether intelligent belief in the gods was increasing. Perhaps this was not the case at Rome.

(*d*) That other religious ceremonies, repeated from age to age, have the same tendency as prayers to become dead forms from which the spirit of communion or devotion has departed—though under favourable conditions it may return from time to time—is too well known; and if in their emptiness they are still believed somehow to serve their purpose, it can only be as magical rites. One may be surprised to find at what an early stage of culture this tendency is fully realised. William Ellis, the celebrated missionary, says of the people of Raiatea: "The efficacy of their [religious] services consisted in the rigid exactness with which sacred days were kept, and religious ceremonies performed, without the least regard to the motives and dispositions of the devotees. ...In their idol-worship, however costly the sacrifice, and however near its close the ceremony might be, if the priest omitted or misplaced any word in the prayers, or if his attention was diverted by any means so that the prayer was broken, the whole was rendered unavailing: he must prepare other victims and repeat his prayers over from the commencement[1]."

(*e*) In Magic there must be something deeply satisfying to the average mind: it precedes Religion, supplies the basis and framework of religious practices, and remains when Religion is in ruins; and when people change their Religion, they retain their Magic. Among the Tongans, those who were Christianised lost their dread of witchcraft last of all the relics of their heathenism. Among the Cherokees, "Gahuni, like several others of their Shamans, combined the professions of Indian conjuror and Methodist preacher[2]." In Norway, after the general acceptance of Christianity, Lapland witchcraft was still valued. The victory of the insurgents at Stiklestad, where St Olaf fell, was thought to have been due to the magic armour of reindeerskin that Thore Hund had brought from Lapland; though all St Olaf's men wore the cross upon helmet and shield[3].

Since then the spiritualising of Magic and the despiritualising of Religion are both real processes of evolution, it may be difficult, or even impossible, to say of a particular magical practice, without particular knowledge of its history, whether it is primitive or residuary. Sir E. B. Tylor writes: "Charm formulas are in very many cases actual prayers, and as such are intelligible. Where they are merely verbal forms, producing their effect on nature and man by some unexplained process, may not they, or the types they were modelled on, have been originally prayers, since dwindled into mystic sentences[1]?" The circumstances of each case must guide our judgment.

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[1] *Polynesian Researches*, II. 144 and 157 (1st ed).

[2] J. Mooney, "Sacred Formulas of the Cherokees" in *Am. B. of Ethn.* VII. 314 (1885–6).

[3] *Heimskringla, St Olaf's Saga*, cciv and ccxl.

V. SPIRITS KNOW MAGIC, TEACH IT, AND INSPIRE MAGICIANS.

Ghosts know Magic, because they knew it in the flesh; and, by analogy, similar knowledge is likely to be attributed to spirits that are reputed never to have been in the flesh. As fear exalts all the powers of a ghost above his former reach, it may be expected to raise his magical powers, especially if he had already been famous in that way. And, generally, it does so; but, exceptionally, we read that, among the Lengua Indians (west of the Paraguay), whilst any man may attempt Magic, professional "witch-doctors" are numerous and powerful; yet they are not credited with extraordinary powers after death[2]. Elsewhere, however, the dead magician does not forget his art. Where Shamanism prevails and the power of Magic or Sorcery attains its greatest social importance, the spirit of a dead shaman makes some advance toward deification. Among the Buryats, dead shamans are worshipped with prayer and sacrifice[3]. According to the *Kalavala*, the famous collection of Finnish poetry, in Tuonela (Hades), whither all dead shamans descend, their wisdom and magical power accumulate, exceeding that of any living adept; so that even Väinämöinen, the wizard hero, goes down to learn there the magical words he does not know[4].

Spirits, knowing Magic, also teach it, and make magicians and prophets. In South-east Australia, the profession of wizard may be hereditary in the eldest son; or obtained through initiation by another wizard—(a corpse is dug up, its bones pounded for the neophyte to chew; he is plastered with excrement, etc., till he becomes frenzied, his eyes bloodshot, his behaviour maniacal); or a man may become a wizard by meeting a spirit, who opens his side and inserts quartz-crystals, etc.; or by deriving power from Daramulun; or by sleeping at a grave, where the deceased opens him, and takes out and replaces his bowels[5].

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[1] *Prim. Culture*, II. 273 (2nd ed.).

[2] S. H. C. Hawtrey, "The Lengua Indians," *J. Roy. Anthrop. Inst.* 1901.

[3] "Shamanism," *J. Roy. Anthrop. Inst.* XXIV. (1894–5).

[4] Comparetti, *The Traditionary Poetry of the Finns*, 184.

[5] Howitt, *op cit.*, 404.

Here we have a list of the most usual ways in which magical powers can anywhere be acquired—by inheritance, by tuition, by the aid of ghosts or spirits; and it suggests the hypothesis that at first the magic art was inherited, or learnt from a former wizard; and that, with the growth of Animism, it became in some cases preferable, because more impressive (and cheaper), to acquire it from a ghost or spirit. For this is more probable than that, at the early stage in which Animism exists in South-east Australia, retrogradation should have taken place; so that the making of wizards, formerly ascribed only to spirits, should in some cases have been remitted to inheritance or to professional tuition. That in spite of the greater prestige that may attach to a diploma obtained from spirits, the right of practising by inheritance or by tuition often still persists, is to be understood by considering family and professional motives. There are heavy fees for teaching witchcraft, besides the profits made in some tribes by selling the control of familiar spirits; the profession is lucrative, and a wizardy family has an interest in its monopoly; which must be impaired, if any man who loses himself in the bush may come back with some cock-and-bull story about a ghost and his new metaphysical insides, and straightway set himself up with the equivalents of a brass plate and a red lantern. Among the Boloki on the Congo, the careers of blacksmith and witch-doctor are open only to the relatives of living adepts. At least, practically (but for a few exceptionally cunning and rascally interlopers), the office of witch-doctor is hereditary: a father trains his son, and will train (for a large fee) any youth whose family has already produced a witch-doctor. But an applicant without family connection is told that he must first kill by witchcraft all the members of his family, as offerings to the fetich of that branch of the profession to which he aspires[1].

Not only the spirits of primitive Animism, but likewise the gods of maturer Religions, know and teach Magic. In the Maori mythology, Tumatauenga, one of the first generation of gods, determined incantations for making all sorts of food abundant and for controlling the winds, as well as prayers to Heaven suited to all the circumstances of human life; and the god Rongotakawin, having shaped the hero Whakatau out of the apron of Apakura, taught him magic and enchantments of every kind[2]. Professor Rhys tells us that the Welsh god Mâth ab Mathonwy, or Mâth Hên (the ancient), was the first of the three great magicians of Welsh Mythology; and he taught Magic to the culture hero, Gwydion ab Dôn, with whose help he created a woman out of flowers[1]. The Teutonic equivalent of Gwydion is Woden, or Odin; and he too was a magician, "the father of spells," who acquired his wisdom by gazing down into the abyss, whilst he hung nine nights on the tree, an offering to himself (and in other ways); and, in turn, he teaches Siegfried the omens[2].

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[1] J. H. Weeks, *Among Congo Cannibals*, 145 and 276.

[2] Grey, *Polynesian Mythology*, ch. i. and vii.

Spirits also inspire or possess the magician, so that through him, as their mouthpiece or instrument, prophecies are uttered or wonders wrought. We have seen (*ante* p. 303) that in South-east Australia the rites of initiation to wizardry by a wizard, without the aid of spirits, cause a candidate to become frenzied or maniacal. With the growing fashion of animistic interpretation, such behaviour is (along with insanity) put down to possession by a spirit. The common beliefs that a man's soul can slip in and out of him and that a man may reincarnate the spirit of an ancestor, facilitate this idea of possession. Dreams concerning spirits also promote the belief that a miracle-monger owes to them his supernatural powers. The Tunguses of Turnkhausk say that the man destined to be a shaman sees in a dream the devil performing rites, and so learns the secrets of his craft. Among the Trans-Baikal Tunguses, he who wishes to become a shaman declares that such or such a dead shaman appeared to him in a dream, and ordered him to be his successor; and he shows himself crazy, stupefied and timorous. The Yakut shaman is preordained to serve the spirits, whether he wishes it or not: he begins by raging like a madman, gabbles, falls unconscious, runs about the woods, into fire and water, injures himself with weapons. Then an old shaman trains him[3]. On the Congo, a man may become a wizard by claiming to be the medium of a dead man; and a medium falls into a frenzy, shouts, trembles all over, his body undulates, sweat breaks out, foam gathers on his mouth, his eyeballs roll: he speaks an archaic language if he knows one[4]. In Santa Cruz (Melanesia), prophecy is practised by men whose bodies are taken possession of, and their voices used, by ghosts: they foam at the mouth, writhe, are convulsed as if in madness; and the mad, too, are believed to be possessed[5]. Similarly the Pythonees: the behaviour of the possessed is everywhere the same. But as the same behaviour marks the youth undergoing training for a wizard before the theory of possession or inspiration has been adopted, it is plain that the animistic theory does not create the phenomena, but is merely, at a certain stage, the inevitable explanation of them.

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[1] *Origin and Growth of Religion as illustrated by Celtic Heathendom*, 225.

[2] *Corpus Poeticum Boreale*, 24, 181, 196, 34.

[3] "Shamanism," *J. Roy. Anthrop. Inst.* x. xiv 85.

[4] Weeks, *Congo Cannibals*, 265.

[5] Frazer, *Belief in Immortality*, 388.

Facility of falling into frenzy may be the test of fitness for wizardry; the Bokongo professor who trains a pupil, beats his drum, shakes his rattle, and tries to drive the fetich-power into him; if the pupil remains stolid, he is disqualified; but if he sways to the music of the drum, jumps about like a madman, etc. he passes[1]. These antics, at first a means of astonishing the beholder, strengthening the faith of patients in the witch-doctor, and of the witch-doctor in himself, and often having a sort of hypnotic fascination, gain in value under Animism by being also proofs of supernatural assistance or control: and being an essential mark of the adept at certain stages of the art's development, they are sometimes induced by rhythmic drumming, singing and dancing, sometimes by fasting or drugs.

'Black Magic' is, at first, merely the use of Magic for anti-social purposes; very early a distinction is recognised between wizards who cause disease and those who cure it[2]. 'Black' and 'white' wizards are sometimes at open strife[3]. When tribal gods are recognised, 'black' wizards are those who are assisted or inspired by inferior gods or demons, who may be opponents and rivals of the high gods. Hence the same god who, whilst paramount, aids or inspires in an honourable way, may, if deposed or superseded, become the abettor of Black Magic—as happened to our own gods, and to others, before and after the coming of Christianity; for the divine sources of power and prophecy became devils and witch-masters. The magicians of our Middle Ages, of whom Faustus is the type, were 'black' and, in the spirit of Shamanism, pretended to rule the devils; but, overshadowed by Christianity, they—at least in popular belief—bought their power at a price.

VI. SPIRITS OPERATE BY MAGIC.

Spirits may operate through men whom they possess, or by their naked soul-force, or by words (that is, by spells), or by merely thinking:

(*a*) When a man is possessed by a spirit, it is the soul-force of that spirit which has entered him and taken command of his voice or limbs;

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[1] Weeks, *Primitive Bokongo*, 215.

[2] Spencer and Gillen, *Native Tribes of Central Australia*, 480–8.

[3] "Shamanism," *J. R. A. I.* XXIV. 130.

and we have seen that this soul-force is the same as force magical.' The spirit's action is the same as that of the *bugin* or wizard, who boasted of having entered a horse and galloped off[1].

(*b*) By Animism, prior to philosophical reflection, the spirit is not conceived of as strictly incorporeal; its force, which is magical, is quasi-mechanical. Hence, in South-east Australia, spirits can carry off a man in a bag[2] (made, no doubt, of bag-soul-stuff). But spirits may act upon a man very effectually without being mechanically felt; as among the Ekoi, where ghosts are either good or bad, and generally a good goes with a bad one to counteract his malevolence; but should a bad one wander forth alone, and should a man without the gift of seeing ghosts (which depends upon one's having two extra eyes) run against it in the street, the ghost will not step aside, but strikes the man in the face; who then has lock-jaw, and dies[3]. As we have reason to believe that this is not the natural aetiology of lock-jaw, the ghost's action is plainly magical: like that of the corpse-candle which, not long ago, on a slope of Plinlimmon one rainy night, a man ran against inadvertently, and was "struck down dead as a horse[4]." The mere apparition of a ghost (at least, to anyone who has not four eyes) is magical. The sending of a bird as an omen is magical.

This immediate power of the gods is nowhere shown more emphatically than in their metamorphoses: that these are sometimes wrought by spells or other enchantments proves that the operation is magical. Australian wizards transform themselves into kangaroos and other animals; and, in Arunta mythology, in the earliest Alcheringa (period of mythical ancestors), the Ungambikula—so called from having arisen out of nothing—with stone knives cut men out of rudimentary masses of unorganised matter (inapertua), and then transformed themselves into little lizards[5]. So this sort of self-conjuring may be said to begin at the beginning; and it cannot be necessary to accumulate examples of metamorphosis.

Several explanations of this belief in the possibility of changing the form of one's body, or of having it changed by others, have been offered: none perhaps entirely satisfactory. We are not here concerned with the passing of a soul from one body to another: given the conception of a separable soul, that is easy to understand. What has to be explained is the belief in a magical change of the body itself, as in the common European superstition that a man may turn into a wolf, and back again. It has been pointed out (i) that the savage may observe striking changes in nature: as in the shape of clouds and smoke, the burning of wood into fire, smoke and ashes, the evaporation of water; the turning of eggs into caterpillars, reptiles, birds, or of a chrysalis into an imago; the appearance of worms in putrefying bodies, and so forth[1]. With such facts before him, why should not the savage imagine himself also capable of transformation? (ii) Dream-images, too, pass one into another in the most marvellous way. (iii) Since men are often called by the names of animals, how easy to suppose that, at times, they may really be those animals. How easy to confound a man with his Totem. In many savage dances, animals are imitated, and the imagination-belief in the reality of the pantomime grows very strong. (iv) The savage, when his imagination has been excited, is not clever at penetrating conjuring tricks and disguises; and some men, at first for their own ends, may have disguised themselves as animals and passed as animals; and in support of this explanation it may be observed that the animal into which men transform themselves is oftenest the most feared in their neighbourhood—the wolf, leopard, or tiger; and, of course, one case believed in, others follow by analogy. The mere report of such an happening might generate belief by force of fear. (v) In a wild country, a man (say one who is pursued) often disappears and is indiscoverable; so that he may seem to have turned into a kangaroo, or a stone, or a tree that appears in his place, as Daphne hid successfully in a laurel-thicket: or if such an occurrence did not originate the belief in metamorphosis, it may have helped to confirm it. Weighing all these hypotheses, I lean to the view that, starting from the fact (as ground of analogy) that astonishing changes are observed in nature and in dreams, the belief in metamorphosis as a magical operation rests chiefly upon the deceptions and confident assertions of wizards that they can, and do, change their form, supported by their reputation for wonder-working and the fears of their neighbours. To the possible objection that the belief is too widespread to have had such an origin, it may be replied that it is not spread more widely than deceit and credulity are[2]. Now, if wizards can change their shapes, of course the gods can.

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[1] Howitt, *op. cit.* 374.

[2] Howitt, *op. cit.* 437.

[3] P. A. Talbot, *In the Shadow of the Bush*, 230.

[4] G. Borrow, *Wild Wales*, c. 88.

[5] Spencer and Gillen, *op. cit.* 388.

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[1] H Spencer, *Principles of Sociology*, § 55.

[2] Cf. E. B. Tylor, *Primitive Culture*, I. 308 *et seq.* The author mentions that in mental disease, the patient sometimes believes himself to be some animal, and acts accordingly—perhaps as a result of the popular belief in metamorphosis, but doubtless also confirming it.

(*c*) Spirits and gods, of course, use amulets and talismans, not invented by poets as symbols, but prized as the instruments of their power, as an enchanter does his wand. Such are the caduceus of Hermes, the cestus of Aphrodite, Thor's hammer Mjölnir, Woden's spear Gunguir and his wishing staff. The gods of Egypt and Babylon also wore charms. Since chieftains are frequently magicians, and also become gods; it follows that the gods are magicians; though, indeed, as Grimm observes, their power is to be called miraculous rather than magical. But Magic, being the highest power known to men, and the most desired, is of course attributed to spirits and to gods.

The most extensive powers of spirits, however, depend on the use of words or spells. The hero of the Western Isles of Torres Straits, Kwoiam, employed magical formulae[1]. The gods and demigods of the Maories carried out their extraordinary adventures by the power of incantations. Maui, by incantations, fishes up dry land from the bottom of the sea, and turns his brother-in-law into a dog; Tawhaki and his brother Karahi, by incantations, make themselves invisible, and avenge their father Hema upon his enemies; and so forth[2]. Celtic and Teutonic deities worked wonders by songs and spells. Isis was the greatest enchantress that ever lived. She made from the spittle of Ra a serpent that bit and poisoned him; and then she healed him by an incantation, having first compelled him to reveal to her his name, to the knowledge of which the god himself owed his power over gods and men; so that she became the mistress of all the gods[3].

As spells, when used by men, may be more efficacious when muttered and whispered than when spoken aloud, so they may retain their power when silently wished or thought; and it is the same with spirits: to control events it may be enough for them to think. And this belief emerges at no very high level of Animism; it needs no philosophical education. The Sia Indians (North Mexico) have a Cougar Society, which meets for a two days' ceremonial, before a hunting expedition, to propitiate the cougar (puma), because he is the great father and master of all game. He is believed to draw all kinds of game to him by sitting still with folded arms and mentally demanding their presence; and by the same means he sends game to whomsoever he favours[4].

Apparently, then, Magic is an art antecedent to the existence of spirits and ready for their use; and they stand in the same relations to it as men do. Animistic usages are originally magical—spells, rites, metamorphoses; and all animistic ideas are magical, except one—the capriciousness of spiritual agency.

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[1] Haddon, *Camb. Exped. to Torres Straits*, v. 329.

[2] Grey, *op. cit.* "Legends of Maui and Tawhaki."

[3] Wiedemann, *Religion of the Ancient Egyptians*, 54–8.

[4] *Am. B. of Ethn.* "The Sia" by Miss M. C. Stevenson, XI. (1889–90), 118.

VII. SPIRITS ARE CONTROLLED BY MAGIC.

The savage imagination having created out of dreams and other strange experiences a world of invisible and powerful beings who may be friendly or hostile,—so human that they must be accessible to prayers, but often turn a deaf ear to them—must desire sacrifices, yet often reject them—capricious and inscrutable—it became necessary, in order to restore confidence in all the relations of life, that their caprice should somehow be overcome; and to accomplish this three ways were open: first, to increase the prayers and sacrifices until their importunity and costliness should prove irresistible,—and this way led to all the magnificence and to all the horrors of religious rites; secondly, to work upon the fears or vulnerability of spirits by beating, starving, slaying, banishing or degrading them; or, thirdly, to constrain them, as men are often constrained, by magical rites and formulae. From the beginning this necessity is felt.

The constraint of spirits by fear or violence is characteristic of Fetichism. The wizards of the Congo catch spirits in traps; or drive them into animals, which they behead; or spear them in some dark corner, and then exhibit their blood upon the spear-head. With such crude practices, however, we are not now concerned.

The control of spirits by Magic, especially by spells—or by other spirits who, in turn, are controlled by spells—is in its earlier form characteristic of Shamanism: indeed, it is the essence of Shamanism; though, of course, in many shamanistic tribes, having intercourse with peoples of different culture, other beliefs, ascribing independent or even superior power to spirits, are often found. Spirits may be so completely subdued by spells as to excite little fear. Among the Yurats and Ostyaks, the shamans treat their spirits without ceremony, and even buy and sell them[1]. So do the Eskimo angekoqs. In Greenland, "all phenomena are controlled by spirits, and these spirits are controlled by formulae or charms, which are mainly in possession of the medicine-men, although certain simple charms may be owned and used by anyone." Hence, "nothing like prayer or worship is possible[2]"; for why supplicate spirits whom you can command? "The rule of man—not of all men, but of one specially gifted (the shaman), over Nature, or over the superior beings who direct her, is the fundamental idea of Shamanism[1]." The shaman's power depends on knowledge of the names, natures and origins of all things and spirits, and of the words that control them; but also on his own extraordinary personality, as manifested in orgiastic frenzy. Megalomania, the vain imagination of being a "superman," is generally characteristic of magicians. Nothing can be more contrary than this attitude to what most of us understand by Religion.

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[1] "Shamanism," *J. Roy. Anthrop. Inst.* xxiv. 133.

[2] Stephánsen, *My Life with the Eskimo*, 391.

One condition of the prevalence of Shamanism among any people, or group of peoples, seems to be the absence from among them of chieftains who have attained to any high degree of political power, and the consequent non-existence of authoritative gods. Hence it spreads throughout the tribes of northern Europe and Asia, from Finland to Kamptschatka, and with a less intensive sway amongst the Indians of North and South America. Under such conditions the shaman is subordinate to no one in this world; nor, therefore, in the spirit-world. But where there are authoritative chiefs, authoritative gods correlative with them are approached by an order of men who are priests rather than magicians,—that is to say, are regarded as dealing less in magic than in prayer and sacrifice. And this state of affairs is apt to give rise to increasing pomp and extravagance of worship, to which there is no visible limit; so that in some cases, as in Ashanti and Mexico, a sort of national insanity has been established. For from such practices there results no security in the satisfaction of desire; the caprice of the gods cannot by such means be overcome; their appetite grows by what it feeds on.

Now, in political affairs something similar happens: the caprice of despotic rulers becomes intolerable; and, in some countries, submission to their tyranny has amounted to a sort of national insanity. Elsewhere devices have been adopted to limit the power of rulers. Avoiding assassination or revolution, it has been found possible to impose upon a king restraints derived from his own sanctity and divine power. One such device has been to surround him with innumerable taboos which, at length, prevent him from doing anything. It is true that the ostensive reason for this was not the limitation of his power, but the preservation of his vitality; and probably this was, at first, the conscious purpose; but one effect of it was to limit his power, and the utility of this was its natural sanction. There are many cases in human life in which a great advantage has been gained for the race by means which were intended by the conscious agents to have an entirely different result[1]. In several countries, where the king has been bound by taboos, another man has by some pretext usurped his power; so that this way of restraining despotism is not a good one. But in Japan, where it had been adopted by a political people, the Tycoon, who succeeded to the power of the taboo-burdened Mikado, himself fell at last under equivalent restrictions, whilst affairs were directed by his ministers. Such is the natural tendency of this device amongst positivists, like the Japanese; elsewhere it may transfer the regal power to warriors or to priests[2]. Another way of restraining the king is to establish the principle that he rules by the laws, and that laws, though made by himself, cannot be altered. And this may have been the purpose of the unchangeableness of the laws of the Medes and Persians; and according to the *Book of Daniel*[3] it was used in this way; though, certainly, the older authority of Herodotus[4] shows that, in some cases, the king's advisers could find a way out for their master. Our own forefathers were no doubt the wisest people that ever lived; and their plan was to acknowledge fully the divinity that doth hedge a king, to declare that, in fact, he could do no wrong, and then to visit all the iniquities of government upon his ministers.

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[1] D. Comparetti, *The Traditional Poetry of the Finns*, 26.

If kings need restraint, much more do invisible gods: and many nations have sought to limit their prerogative, either by Magic or by legal fictions which, in relation to gods, can have only a magical operation. In Egyptian rites of sacrifice and prayer, the kind of victim and the manner of slaying and cutting it up were minutely and unchangeably decreed. "The formulas accompanying each act of the sacrificial priest contained a certain number of words, whose due sequence and harmonies might not suffer the slightest modification even by the god himself, under penalty of losing their efficacy. They were always recited with the same rhythm, according to a system of melody in which every tone had its virtue, combined with movements that confirmed the sense and worked with irresistible effect; one false note, a single discord between the succession of gestures and the utterance of the sacramental words, any hesitation, any awkwardness in the accomplishment of a rite, and the sacrifice was vain[5]." But if all was in order, the god was bound to grant the petition. Babylonian religious ceremonies "had for the most part the same end and object as the magical text used with them; they were not so much a communion with the deities of heaven, as an attempt to compel them by particular words to relieve the worshipper from trouble, or to bestow upon him some benefit." Ceremonies, therefore, were useless unless accurately performed in word and deed; "ritual was a sort of acted magic[1]." These accounts of the religious ceremonies of the highest barbaric civilizations are almost in the same words as William Ellis uses in his account of worship at Raiatea about the beginning of the nineteenth century; except that Ellis does not say that the Polynesian gods were bound to grant the requests so presented. Accordingly, I have treated the Raiatean example under Retrogradation, and those of Egypt and Babylon as cases of half-conscious policy. No doubt retrogradation and policy were present in all cases; but it seems reasonable to suppose that the latter predominated where order was more settled (an analogue of the order required in heaven) and thought was better trained.

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[1] *Natural and Social Morals*, c. ii. § 4.

[2] See many examples in J. G. Frazer's *Taboo and the Perils of the Soul*, c. i.

[3] c vi.

[4] III. 31.

[5] Maspero, *The Dawn of Civilization*, 124.

One may wonder why a magical ritual should be preferred and trusted rather than genuinely devotional worship; since it must, in fact, just as often result in disappointment. But, first, as to the priesthood, an elaborate ritual, difficult to carry out, is favourable to their power, because only professionals can execute it; so that they must necessarily be employed; and the more elaborate and exigent it is, the more necessary they are. But, then, the more attention the ritual demands, the less there is to spare for thinking of the gods. Secondly, as to the people, since the failure of worship in attaining our ends may be due either (animistically) to the caprice of the gods or (magically) to an error of the priest, it is not surprising that men should trust the specialist whose education is well attested rather than the god whose character is inscrutable. Thirdly, a magical ritual appeals to the expectation of uniformity, the sole ground of confidence concerning the future, and therefore what men most desire[2]. Nevertheless, the religious form of the rites (though empty of religious feeling) is maintained; partly, because the whole political and ecclesiastical fabric rests upon the animistic tradition; partly, because Animism has such hold upon men's minds that a few remain devout; whilst even those who regard the rites as magical do not perceive that magic is the antithesis of religion and rigidly excludes it. Only a few natural positivists and philosophers regard public worship as merely a political institution.

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[1] A. H. Sayce, *Religion of the Ancient Babylonians*, 319.

[2] At the Meeting of the Psych. Soc. (Jan. 29) after this paper had been read, Dr Ernest Jones read a paper on Symbolism. He showed (amongst other things) how a symbol may supplant the thing it represents, and become for consciousness independent, through affective inhibition; because that which is represented is the less agreeable idea. This helps us to understand how the magic character of a religious rite supplants the animistic, namely, because the uniformity of magical tendency is a more agreeable idea than that of spiritual caprice. The latter idea is therefore inhibited. Similarly (as observed above, p 293) spirits may be believed in without being prayed to, because such disagreeable neighbours are thought of as little as possible.

The idea of a transaction by which the gods are legally bound—so much help for so much worship—may be present in all magical ritual; but in some religions the analogy of human relations according to law is explicitly extended to the relations of men with gods. The Jewish religion was based on a covenant; and, according to some theologians, so is the Christian. It has often been said that Roman religion implied a belief in legal obligation imposed upon the gods by rites duly performed; and Mr Warde Fowler, who thinks more highly than some have done of the genuineness of religious feeling amongst the Romans, at least in private worship, yet says that in the *vota publica* we find something like a bargain or covenant with the deity in the name of the State[1]. Legal obligation involves effectual sanctions that may be brought to bear upon transgressors, gods or men; and at a low stage of Animism, when no spirit exceeds the rank of demon, there may be no incongruity in bringing to reason a recalcitrant spirit by stopping his rations or maltreating his image; but when high gods have obtained the homage of men, to punish them calls for great audacity or very subtle management. The Chinese have managed the matter to admiration. The Emperor of China acknowledged himself subject to the spirits of Earth and Heaven; but he himself was the son of Heaven, and all other spirits were subject to him. He ruled alike over the dead and the living. He made deities and appointed them their functions; promoted them and distributed amongst them titles of honour, if they did good works; or degraded them, if they failed in their duties. In the *Pekin Gazette* one finds "the deities figuring, not occasionally but very frequently, in every department of official business, and treated much as if they were highly respectable functionaries of a superior order, promoted to some kind of upper house, whose abilities and influence were nevertheless still at the service of the State[2]." Nowhere has the unity of Church and State been so completely realised, and the *pax deorum* so conclusively established. One may interpret the facts at discretion: an animist may accept them literally and seriously; a devotee of magic may regard decrees in the *Pekin Gazette* as spells that have coercive power in the spirit-world; a Confucian mandarin will think that an excellent plan has been devised for enlisting the superstitions of the simple-minded in support of law and order. We may suppose that for him Animism is but an episode in the history of human thought.

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[1] *Religious Experience of the Roman People*, 202.

[2] Alfred Lyall, *Asiatic Studies*, essays on *The State and Religion in China.*

Another way of excluding spiritual caprice, which we might suppose to have been discovered by philosophers, but which appears to be older than what we usually call 'Philosophy,' is to subordinate the gods to Fate. The idea has been attributed to the astronomers or astrologers of Babylon that Fate must be above the gods as the constant heaven of the fixed stars is above the planets[1]: an analogy characteristic of magical thought. But the roots of the idea of Fate are much older and wider spread in the slow, steady growth of the belief in uniformity, which is the common ground of Magic and Science; and (as I have said)[2] before laws of nature have been discovered, Fate is an all-comprehensive Magic. Fate reduces the gods to the status of wheels in a machine; omens and oracles, instead of being sent or inspired by the gods, are also part of the machinery, and may point to their destruction; prayers and sacrifices are other parts of the machinery and, at most, may be a means of assuaging the anxiety of one's own heart. A stern way of envisaging the world: but it gives not only security against the gods, but also resignation and tranquillity.

Philosophical Christianity regards the actions of God as always manifested, in the physical order, through 'second causes' or, in other words, in 'the laws of nature'; and, in the spiritual order, as always observing the moral laws that are the principles of divine Reason; in either case there can be no variableness nor shadow of turning.

Magic, like Science, believes in uniformities of nature, and seeks by a knowledge of them to control events; but Magic is so eager to control events that it cannot wait to learn the true uniformities; it is not moved, like Science, by curiosity as to the truth, but by blind desire for present results. The cult of spirits seeks to control events not by knowledge of their natural causes, but by appealing to hyperphysical causes, and resembles the belief in Free Will, by which men hope, through the influx of some unknown energy, to escape the bondage of their own vices: for Kant rightly treated 'Freedom' as a cosmological problem, the supposed intervention of a cause that is transcendent and not in the course of nature. The intervention of Free Will (whether divine or human) is sought in order to avert injurious fortune, to realise our personal or social schemes more quickly and cheaply than our own efforts can, to avoid the consequences of our own actions, amongst which is bondage to our own vices: for all these, give us variability, miracle, caprice. But to foresee and control events physical or social, including the conduct of others, to be confident in the effects of our own actions according to our purposes, and in the stability of our own character: for all these, there must be uniformity. In the long run the latter considerations determine our thoughts; and the necessity of uniformity to a rational life may be one cause of our belief (so far out-running the evidence) in uniformities of causation and of space-relations and of all that we mean by natural law.

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[1] Franz Cumont, *Astrology and Religion.*

[2] *Ante* p 301.

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THE PREVALENCE OF SPATIAL CONTRAST IN VISUAL PERCEPTION.

By W. G. SMITH.

The aim of the investigation.
Details regarding the method employed.
Discussion of the results; complexity of the factors involved in their interpretation.
Special features of the determinations.

It is the object of this paper to present observations regarding the modifications in spatial perception which are introduced when a line in the visual field, whose length is being estimated, is accompanied by another line parallel to it and of varying length. The problem is of interest on two grounds: it presents one of the simplest cases in which contrast can appear, and it comes into close relation with phenomena of ordinary experience in which the influence of contrast is usually supposed to be quite evident. An account was given in an earlier paper[1] of experiments which were directed in part to the study of this topic. The results obtained on that occasion were sufficiently definite, and appeared to render superfluous the employment of contrast as an explanatory condition. Since, however, the data in that investigation were supplied by two subjects, who at the same time were conducting the experiments, it appeared desirable to secure observations from a larger number of subjects, who also should be ignorant of the special significance of their work at the time when the determinations were being made.

Observations fulfilling these conditions were obtained from members of the class of Psychology in the University of Edinburgh in the course of work carried out during the Summer Term, 1913. From those who took part in the work forty were selected, twenty of these being men and twenty women; the ground of choice lying in the satisfactory character of the work which was done in following out the procedure indicated to the class. It is the results obtained from these subjects which are submitted to examination in the following pages. By the time the determinations were made a fair amount of training in other lines of experimental work had already been gained by the subjects, and the work on contrast followed immediately on that which had been given to the study of the Müller-Lyer illusions in visual space perception. It may be mentioned that the measurements involved in working out the results which are to be dealt with in this paper were carried out by the author[1]. In its general character the method of reproduction, employed on the present occasion, was the same as that of the earlier investigation to which reference has already been made. The following are the special features of the procedure.

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[1] This *Journal*, 1907, II. 196.

The material which was used in the experiments consisted in the first place of cardboard sheets of uniform size on which were printed the lines which presented the relation of contrast and connected features. The size of these sheets, which may be termed conveniently 'pattern sheets,' was 12 by 8 inches; the breadth of the lines, which was the same throughout, was slightly over $\frac{1}{32}$ in. On all the sheets there appeared a line 4 in. in length, which we shall term the 'constant line.' On one set of sheets this constant line appeared without any accompanying line; on the other sets it was accompanied by another line lying parallel to it at a distance of half an inch. In the first or 'normal' case we have the means of securing data regarding normal estimation; in the other 'special' cases the influence of the accompanying line may be studied. The lengths of the accompanying lines in the special cases, seven in number, were the following:—1, 2, 3, 4, 5, 6 and 8 in. The single line in the normal case and the pair of lines in the other cases lay parallel to and midway between the two longer sides of the pattern sheet. The constant line was placed in all cases so as to have one end distant 2 in. from one of the short edges of the sheet; where there was an accompanying line it had one end placed similarly with respect to the same edge. There were prepared in addition sheets of ruled foolscap paper, on which was printed a vertical marginal line distant approximately $1\frac{1}{4}$ in. from one side of the sheet—the side which was to lie towards the left hand of the subject.

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[1] A report presenting some of these results was included in a paper on "Contrast as a factor in psychological explanation" which was read in the Psychological Subsection at the meeting of the British Association in 1913.

Following the directions given at appropriate points in the course of the work, each subject placed on the left hand a pattern sheet with its longer edges parallel to the edge of the table at which he sat, and on his right hand (in contact with the pattern sheet) a sheet of the prepared foolscap paper. In carrying out each determination the subject looked at the constant line as long as he wished; then without looking back he made on a line of the sheet lying on his right a mark in ink at a point such that the distance between it and the intersection of the horizontal and vertical lines on the left seemed equal to the length of the constant line. As soon as the mark was made, and without any attempt being made to compare the reproduced and presented distances, the sheet containing the marked line was moved upwards under another covering sheet, a fresh surface being thus exposed for the next determination. The horizontal line which was being marked lay approximately at the same level as the line whose length was being reproduced; the covering sheet was kept in the same position throughout, and by means of an additional underlying sheet the total area of paper exposed remained constant. Eight determinations were made in the normal case, while six determinations were made in each of the seven special cases. A certain variation was introduced by arranging that one division of the subjects, containing ten men and ten women, should have the pattern sheet so placed that the constant line was the upper one of the two parallel lines, while the second, containing the same proportion of subjects, had the lines presented in the opposite position. It may be pointed out that this arrangement permitted one, without drawing special attention to the character of the constant line, to give appropriate directions for the work by referring to the reproduction of the 'upper' or 'lower' line. An additional variation was introduced by making the order of the determinations different in the two divisions indicated above. Referring to the normal case by the letter *a*, and to the special cases, taken in the order of increasing magnitude of the accompanying lines, by the letters *b* to *h*, we may state the difference succinctly as follows. In Division I there were first two consecutive determinations for each case in the order *a* to *h*; next two for each case (except *a* where 4 were made) in the order *e* to *h*, then *a* to *d*; lastly two for each case in the order *h* to *a*. In Division II the order was first *h* to *a*; next *d* to *a*, then *h* to *e*; lastly *a* to *h* It might fairly be expected that these changes, while heightening, perhaps, the variability of the reproductive process, would tend to neutralise the effect of irrelevant conditions and furnish a clearer insight into those relations of the phenomena in which we are now interested

In Table I are presented the results for the divisions taken separately, and also for the subjects taken together for the purpose of forming a general average (Gen. Av.). The various cases are indicated at the head of each of the vertical columns, not merely by the letters already employed, but also by numbers giving the length in inches of the accompanying line. The length of the reproduced lines has been measured in millimetres and fractions of this unit; the length of the line which was to be reproduced was approximately 101·4 mm. in length[1]. With the aim of throwing into relief the two sets of results which may be compared with those where contrast might make its appearance, the *a*–0 and *e*–4 cases have been distinguished by doubled vertical lines. It may be remarked with respect to the general averages that each of the numerical values is based on 240 measurements, with the exception of that for the normal case (*a*–0), where the total is 320.

TABLE I.

	a—0	*b*—1	*c*—2	*d*—3	*e*—4	*f*—5	*g*—6	*h*—8
Div. I	94·1	95·4	94·7	94·4	93·9	94·1	93·4	92·9
Div. II	94·7	95·5	94·9	94·6	94·8	95·3	95·1	94·9
Gen. Av.......	94·4	95·4	94·8	94·5	94·4	94·7	94·3	93·9

If we consider first the general averages we seem to find evidence of the operation of contrast in the two extreme cases, *b*–1 and *h*–8; viz. an increase in the estimated length of the constant line when the accompanying line is shorter than it, and a decrease when the accompanying line is longer. The same is true of the results for the divisions taken separately with the exception of the case *h*–8 in the second division. Taking the other cases, where the difference between the two parallel lines, though not so great, is yet quite obvious, we find on the other hand that there is little or no evidence of modification due to contrast either in the general, or in the more detailed averages. While the values in the case of normal estimation, *a*–0, form the chief basis in the examination of the evidence for contrast, it seems fair to take into consideration also the values in the case *e*–4, which are seen both in this table and in Table II to correspond with great closeness to those in the case *a*–0.

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[1] It should be noted that, had the constant line as printed been exactly 4 in. in length, it would have measured 101·6 mm. approximately. The various accompanying lines as printed were similarly, with slight variations, below the standard length.

It will be observed that, of the modifications, or differences, which might be attributed to contrast, the greatest are those presented by the first division in the two extreme cases, viz. an increase of 1·3 mm. in the case *b*–1, and a decrease of 1·2 mm. in the case *h*–8. A basis for estimating the significance of these and other related values has been sought in the calculation of the probable error of the various averages involved in the determination of the differences. This has been carried out for the cases *a*–0, *b*–1 and *h*–8, and in addition for the case *e*–4. The values of the probable error are given below not merely for Div. I, in which the greatest differences are found, but also for Div. II.

a—0		*b*—1		*e*—4		*h*—8	
I	II	I	II	I	II	I	II
1·1	1·2	1·2	1·5	1·1	1·2	1·1	1·5

As will be seen, the differences between the normal and the modified estimations in the two extreme cases of the first division (*b*–1 and *h*–8) are greater than the probable errors of each of the averages on which the differences are founded. This fact, however, is of comparatively slight significance. It is more important to consider the relation of each difference to the joint variability of the averages involved in its determination[1]. We find then that in no case does the magnitude of the difference approach the sum of the probable errors of the averages on which it is based. It may be suggested that other criteria of variability might be employed, *e.g.* the error of mean square. It may be taken, however, that the use of the probable error is not an unduly severe procedure, and if a more stringent test be desired, a few calculations based on the values presented above will indicate its bearing on the present problem. To the question, then, regarding the significance of the differences which might be attributed to contrast, the answer must be that in view of such a criterion their value is slight, and that they furnish little or no proof of the general prevalence of contrast. Were the effects attributable to contrast shown in a series of differences which though small in magnitude, were continuous and harmonious, it might be urged that some significance should still be attached to them. But the actual series of differences can hardly be said to possess these characteristics in a marked degree.

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[1] Cf. Radicke, Wunderlich's *Archiv f. physiol. Heilkunde*, 1858, N.F. II: *v.* also translation of the New Sydenham Society, 1861, XI.

Reference may be made at this point to the conclusions reached in the earlier investigation of this topic It was then found that there was a decided and regular tendency on the part of both subjects to increase the length of the reproduced line when the constant line was accompanied by another which was longer than it, but that no distinct modification was apparent—at the most, only a slight tendency towards increase—when the accompanying line was shorter. The results of the present investigation appear to be as completely out of harmony with the principle of confluence which was applied in the interpretation of these earlier data, as with the principle of contrast.

A suggestion has, however, been gained for an interpretation, which will avoid merely negative conclusions, in the analysis of the present data from another point of view. When averages are calculated separately for the groups of men and of women (twenty subjects in each group) the following results are found.

TABLE II.

	a—0	*b*—1	*c*—2	*d*—3	*e*—4	*f*—5	*g*—6	*h*—8
Men............	92·5	94·1	93·0	92·2	92 5	92 2	91·5	91·3
Women	96·3	96·7	96·6	96·9	96·2	97·2	97·0	96·5

It is of some interest to notice that in all the cases the average length of the reproduced line is less in the group of men than it is in the group of women. That this feature is not a matter of chance is clear when we consider not merely the persistence, but also the magnitude of the difference in the various cases. As regards the latter point, we find on applying the test already used to the normal case, which we may take as an example, that while the difference between the estimations of men and women amounts to 3·8 mm., the probable error of the men's average is 1·3 and that of the women's average is 1·1, their sum being 2·4.

Passing from the consideration of this feature we note that the group of men presents a series of modifications, which, though not striking in magnitude, are yet fairly uniform and, except in the case *d*–3, are in harmony with the influence of contrast. The group of women, on the other hand, shows a fairly uniform tendency in the direction of increase, whether the accompanying line be long or short, the magnitude of the increase, however, being less in the cases where the accompanying line is shorter than the constant line. In other words there are indications in the latter group of the operation of confluence.

The suggestion might then be made that while the results do not afford ground for asserting the general or unrestricted prevalence either of contrast, or of confluence, they yet give some indications of their presence. In the circumstances of the present investigation these two conditions are probably in part antagonistic. Their relative effectiveness may reasonably be supposed to vary according to the individuality of each subject, and, further, according to the general tendencies of sex. We have already seen that there is a decided difference between the men and women, who took part in this work, with regard to the determination of the lines. One might say then that while contrast is more prevalent among men, confluence is more prevalent among women. The results of the earlier investigation would fall to be explained by a dominance of confluence in the subjects taking part in that investigation. The question may, however, be raised:—if the determinations of the group of women are to be regarded as dominated by confluence, how does it come that this group presents an increase in the cases *b*–1, *c*–2, *d*–3, instead of the decrease which confluence might be expected to introduce? A fully satisfactory answer to this question cannot be given, but reference may be made to certain points. On the ground of observations regarding the magnifying and diminishing forms of the ordinary Müller-Lyer illusion it may be assumed that confluence is less effective in reducing the apparent length of the longer of two parallel lines than in increasing the length of the shorter. In other words, the condition which is antagonistic to confluence will have more scope in the cases *b*–1, *c*–2 and *d*–3 than in the other special cases. We may thus, perhaps, look on the increase of the reproduced line in these cases as an indication of the restricted manifestation of contrast[1]. It may be that the tendency in the group of women towards a relative increase in the length of the reproduced line—or, as we should perhaps say, the tendency on the part of the men towards decrease—has some bearing on the mode in which these tendencies are realised. The results mentioned in the next paragraph may be taken as evidence on this point. Other tendencies, which at present one can only describe as accidental, are doubtless at work also.

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[1] It has been assumed above that confluence is operative both in the ordinary Muller-Lyer illusion and in the analogous case where two parallel lines of different length are presented—a case whose relation to the former is seen if we suppose the ends of the parallel lines joined so as to form distinct angles. Cf. this *Journal*, II. 203, 214. Since the modification in the magnifying form of the Muller-Lyer illusion has been proved to be decidedly greater than that in the diminishing form, it has been assumed further that this involves a difference in the effectiveness of confluence in the two forms. Cf. this *Journal*, II. 22. This difference between the two forms may also involve in a measure the antagonism suggested above Cf. Lewis, this *Journal*, 1909, III. 21.

An attempt has been made by grouping the subjects in various ways to reach conclusions on other points of interest. Such attempts, however, are apt to meet with difficulties owing to the selection involved in the grouping. One may, for example, choose from the subjects those who show a special tendency in some section of the cases. In doing this however we are in fact at the same time making a selection of instances where the reproduction of the lines in the other cases is modified by the opposed tendency. The analysis may thus furnish us with little that is instructive. This may be illustrated by considering the result of selecting those subjects who show a definite tendency to increase the length of the reproduced line in the special cases *f*–5, *g*–6, *h*–8. With this in view a group was formed of the ten subjects (4 men, 6 women) who made the reproduced line in two at least of these special cases greater than that in either of the cases *a*–0, *e*–4. The following are the averages for this group:

a—0	*b*—1	*c*—2	*d*—3	*e*—4	*f*—5	*g*—6	*h*—8
93·0	94·6	94·2	94·4	93·4	95·6	95·8	93·8

It will be noted that the selection has not left the averages for *a*–0 and *e*–4 untouched; they are distinctly less than those of the general average in Table I. In addition the values in *b*–1 and *c*–2 show a corresponding decrease. It may be observed also that we find evidence in the group thus selected of the tendency, already found to be characteristic of the group of women, towards an increase in the reproduced line (when compared with that in the case *a*–0) when the accompanying line is less as well as when it is greater than the normal line.

The characteristic reproductive tendency of the individual subject manifests itself with remarkable uniformity in the various cases and forms one of the dominant facts. This feature is illustrated clearly by the results contained in the following table. The upper and lower series of values present, respectively, the averages for each of two groups of five individuals:—(A) those among the forty subjects whose reproductions of the line in the *a*–0 case stand highest, and (B) those whose reproductions are lowest. We may note how persistently the tendency towards increase or decrease exhibited in the normal case is maintained throughout the special cases.

TABLE III.

	a—0	*b*—1	*c*—2	*d*—3	*e*—4	*f*—5	*g*—6	*h*—8
A.........	108·0	109·2	107·3	109·0	106·1	106·0	106·8	107·9
B.........	80·9	81·0	80·3	80·2	81·8	82·5	81·1	81·1

It will be observed that while there is an increase in the averages of the A group (as compared with the normal line of 101·4 mm.) the corresponding decrease in the B group is much greater, being on the whole more than twice as great. We have here before us the expression in another form of the general tendency towards making the length of the reproduced line less than that of the normal line—a tendency which, as has been seen in Table II, holds both for men and women, though in different degrees.

The mean variation has been determined for each individual in the cases *a*–0 and *h*–8; the average of these mean variations has then been calculated for the groups of men and women taken separately. It has been found that in these cases the values for the men are, respectively, 3·0 and 2·7, while those for the women are 3·3 and 2·5. It would thus appear that, so far as the present investigation is concerned, there is less variability in the reproduction of the modified than in that of the unmodified line, and, further, that the work of the men is somewhat less variable than that of the women.

SUMMARY.

1. There is little or no evidence in the general results of this investigation dealing with parallel lines that contrast is distinctly and generally operative in modifying the apparent length of a line. On the other hand there are certain facts which may be regarded as suggesting that it is present together with other conditions, such as confluence, which in certain situations neutralise its influence but in others permit it to appear.

2. When the subjects are divided into groups of men and women it is found that the results of the former group are more readily interpreted in accordance with contrast, and those of the latter in accordance with confluence.

3. It is found that, while the average length of the reproduced is less than that of the presented line with both men and women, the tendency towards decrease is markedly greater in the group of men.

(*Manuscript received* 10 *March,* 1916.)

A CONTRIBUTION TO THE STUDY OF FATIGUE.

BY MAY SMITH,

(*From the Psychological Laboratory, Oxford.*)

I. *Introduction.*
II. *Description of the Tests and Method of Inducing Fatigue.*
III. *Results of the Several Tests.*
IV. *Effect of Fatigue on a Fatigued State.*
V. *Comparison of the Several Fatigue Cycles.*
VI. *Subjective Effects.*
VII. *Theoretical Considerations.*
VIII. *Practical Considerations.*
IX. *General Summary.*

"FATIGUE is the one field that has been thoroughly ploughed over by science and practical life in the course of the last decade[1]."

Confronted with this optimistic statement the student of fatigue problems, whether from the standpoint of psychological theory or of industrial and scholastic life, is somewhat disappointed when he attempts to survey this voluminous literature, for, of anything approximating to positive evidence there is but little, while the final issue of many of these oftentimes laborious researches permits of no conclusion more general than that, in the words of one of the most recent writers, "signs of fatigue differ according to the individual" or that the tests used as fatigue indexes cannot be regarded as adequate[2].

The present investigation, undertaken at the suggestion of Mr McDougall and carried out under his supervision, is a continuation of some work published by him in this journal in 1905[3]. It is there suggested that one reason for the inadequacy of so many of the fatigue tests is, that there is no guarantee that the subject maintains the same attitude towards the task throughout the investigation; and, even if, as with the Kraepelin school, interest is kept at a minimum, there is still the difficulty that there is no criterion of this minimum interest: boredom, variations of zeal, the stimulus of nearing the end, all militate against the successful measurement of a test. Reversing the usual procedure McDougall argues that a more hopeful plan would be to keep the interest of the subject at a maximum throughout, and use a test of the nature of a sprint, so as "to provide the subject with a task demanding for its execution a continued maximal voluntary concentration of attention."

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[1] Münsterberg, *Psychology and Industrial Efficiency*, 1913.

[2] Cp. Stanley Kent, *Report on Industrial Fatigue*, 1915.

[3] Wm McDougall, "On a New Method for the Study of Concurrent Mental Operations and of Mental Fatigue." This *Journal*, 1905, I. 435–445.

The machine devised by McDougall for this purpose is "a mechanical device whereby a continuous band of paper tape about one inch wide is drawn behind an opening or window in the top of the desk by a weight-driven clockwork movement." The rate at which the tape travels can be adjusted so that the subject works at his maximum speed. Along the width of the band small red circles are distributed in as irregular a manner as possible. The test consists in marking the red circles with a stylographic pen as they pass before the subject's field of vision[1].

The task is too exciting to allow a subject to acquiesce in failure: should failure occur at any stage the interest aroused prompts to renewed effort and not to acquiescence or despair, so that each experiment represents the best work of the subject under the given conditions[2].

But the tests themselves are not the only reason for the negative character of so much fatigue work. Investigators have in many cases set themselves a too difficult problem. That fatigue is produced through daily work is a familiar fact, but it does not follow that the tests which happen to have been selected by experimenters will necessarily be fine enough to detect fatigue on such a small scale as might be engendered, for example, by the working of sums for two hours; while to wait until the exigencies of life produce in the subject unmistakable fatigue is not unlike a physicist waiting for the occurrence of a thunder storm to pursue investigations into electricity.

Expecting less from the test, it was decided to make certain, for experimental purposes, of a fatigued condition; and for this purpose the simplest plan seemed to be to reduce the amount of sleep for a definite time. If the selected test proved adequate as a revealer of fatigue on a large scale then further investigations could be made to see with what degree of refinement it would work.

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[1] For a full description of the machine see this *Journal*, III. 153. Burt, *Experimental Tests of General Intelligence.*

[2] After an almost daily experience of using the machine for three years I yet find it intensely interesting.

The object of this research was to seek answers to the following problems:

1. Is it possible to measure fatigue objectively?
2. What are the immediate effects of fatigue? Do they differ in any measurable degree from the remoter effects?
3. How long does it take to return to a normal condition as estimated by an objective standard after undoubted fatigue, and what is the nature of the recovery curve?
4. What is the effect of fatigue on an already fatigued state?
5. What is the relation between fatigue as experienced by the fatigued person and fatigue as estimated by some objective standard?

Control Precautions.

The writer was the subject of the experiments, and every precaution was taken to keep conditions constant, the fatigue effects to be the only variable; so every day the same length of tape was dotted, and the mistakes counted, for several weeks before inducing fatigued conditions, and continued after the return to a normal condition, as estimated by the number of errors, in order that sufficient data for normal variations should be available: in fact for over three years, except for vacations, the same experiment was performed on an average of five days a week[1].

The experiments too were done at approximately the same time of day: where this was not possible the usual amount of tape was dotted to keep the practice constant, but such records were not included in the results.

DESCRIPTION OF THE TESTS.

1. *The Dotting Machine Experiment.* A length of the tape six metres long was the standard amount. This consisted of 1200 small red circles, and the machine was so adjusted that the circles passed before the field of vision at a uniform rate of 5·8 a second, and each was to be dotted with a stylographic pen as it passed. In each metre fifteen circles at irregular intervals were coloured beforehand and these, when they appeared, were not to be dotted, but instead, the right hand

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[1] Unless investigators have evidence of the normal variations, which all experimenters who have done continuous tests over a long period know to be great, it is useless to assign variations after work to fatigue only.

had to be lifted to avoid marking the circle, while to prevent the subject profiting by these irregular rests, with the left hand a key had to be tapped, which being attached to a pen arrangement under the lid of the machine marked the tape when tapped. By this means a valuable test of the subject's power of inhibition was obtained and also the rather remote possibility of the pattern becoming known was lessened.

It was a simple matter to compute the number of errors in each strip. Errors were reckoned thus: a circle unmarked or a coloured circle marked, or an extra dot inserted between two circles, counted as one error; a lateral or vertical deviation of less than 2 mm. counted as half an error[1].

2. In some of the later experiments, in order to isolate the purely mental factors from the mental and muscular combined which the dotting experiment involves, a modified form devised by Mr McDougall of the associated words test was used[2]. The test required that the subject should appreciate a relationship between words, and secondly be able to reproduce the words in the right sequence. A list of forty words drawn up by a collaborator was read to the subject; the list was so arranged that there was a connection between the first word and the second, between the second and third and so on, a connection which the subject could grasp by an act of attention, *e.g.* a typical list is as follows: mountain, plain, ugly, beauty, Venus, Greece, oil, smooth, rough, rude, cultivated, refined, sugar, sweet, salt, sea, sky, heaven, angel, spirit, brandy, bottle, cork, Ireland, Ulster, cloak, cape, etc. In such a list the subject naturally notes the connection between 'mountain' and 'plain' as geographical terms: but the following word 'ugly' involves the recognition of the meaning of 'plain' in aesthetics; beauty is related as the negative of ugly and the connection with 'Greece' is obvious, but the following word 'oil' demands the realization of the same sound as used in another sense; and so on with the complete list.

It is of course necessary that the writer of the list should keep the material and the relationships within the compass of the subject's experience. If the test is used with educated adults there is no difficulty in framing many lists, as there is a large field of well-known literary, historical and general knowledge upon which to draw: in the case of children greater care must necessarily be exercised, as will also be the case if the social class of the subjects be different from that of the writer of the list.

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[1] With subjects less practised than the writer at this particular test it is quite sufficient to have each circle marked simply without the complications described above.

[2] Cf. Norsworthy, "Psychology of Mentally Deficient Children." Lists of related words were used but all the words of each list were connected with one subject.

The list was read through at a uniform rate of one word in two seconds. After the list had been read through once, the subject, having been given the first word, had to reproduce verbally the complete list in right sequence: failure to give the correct word within ten seconds counted as one error: a wrong word corrected within ten seconds counted as half an error.

3. *The Windmill, an Illusion of Reversible Perspective.* It is a matter of common observation that if one gazes with one eye at the rotating arms of a windmill, while standing at some distance away and a little to one side, the movement of the arms apparently changes its direction from time to time, the movement of the arms in the upper part of their orbit being sometimes towards the observer, sometimes away from him.

A laboratory windmill or revolving cross was fixed up, a description of which has been given by McDougall[1]. Prior to the fatigue experiments considerable individual differences in the number of phases seen in a minute had been found, and this prompted the enquiry into the effect that fatigue would have[2].

4. A few experiments on speed of tapping, for which a modified form of the usual tapping test was used. Four telephone counters as manufactured by the Veeder Manufacturing Co. were mounted on a wooden stand. To each counter is attached a handle which on being pressed registers automatically, on the principle of the cyclometer, that it has been pressed. The test lasted for four quarter-minutes. The numbers registered by the counters were noted at the beginning: the subject at the given signal began to tap as quickly as possible for fifteen seconds on 'counter' 1; at the command 'change' from the experimenter he went on to number 2, the word 'change' being again called at the end of the second fifteen seconds, and so on with the third and fourth. The number of taps made in the minute could easily be computed.

A wooden handle with an india-rubber tip was used as the instrument for tapping the counters.

5. *The Learning and Re-learning of Nonsense Syllables.* This test consisted in learning sets of twelve nonsense syllables under the usual conditions, and of re-learning the same twenty-four hours afterwards.

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[1] McDougall, "Physiological Factors of the Attention Process," *Mind*, N.S. xv.

[2] Cp. Flugel, "Fatigue in Illusions of Reversible Perspective," this *Journal*, VI Pt I.

For the sake of clearness I have briefly summarized here the tests used, but not all were used for the first fatigue "cycle," by which I mean the period covered by the time of shortage of sleep and the return to a reliable normal.

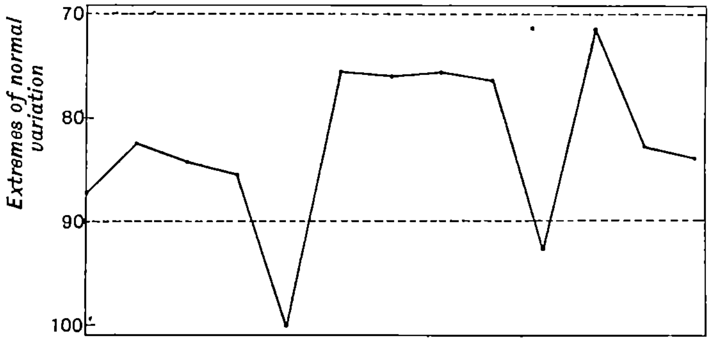
The first experiment was confined to applying the dotting machine as a register of changes in mental efficiency resulting from the fatigue due to loss of sleep.

Method of Inducing Fatigue.

To ensure fatigued conditions the normal[1] amount of sleep was curtailed for three successive nights: on the first night's vigil only one and half hours' sleep were taken, on the second three and a half, and on the third five and a half hours', *i.e.* for three nights less than half of the normal amount of sleep was taken[2].

RESULTS.

1. *The Dotting Machine.* Under normal conditions the number of errors including all types lies between 70 and 90. Those ranging round 70 I looked upon as good normals, those round 90 as bad; by a bad normal I understood a slight deviation due either to extraneous circumstances, *e.g.* an unexpected noise, or to temporary disturbances,



GRAPH 1. A typical fortnight showing daily variations. The two occasions when the records were below normal were due to slight temporary excitement.

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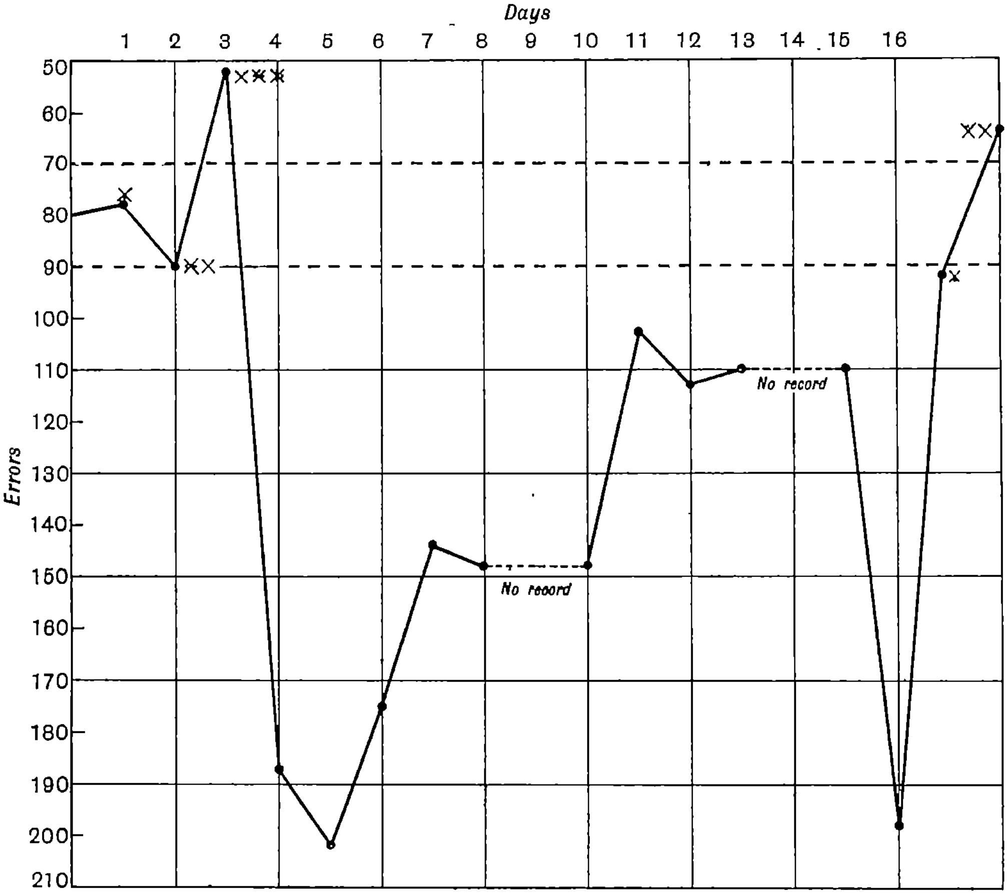
[1] Normally I sleep about eight hours and do not suffer from insomnia. In the experiments described I also fell asleep as soon as my time limit, fixed beforehand, was up

[2] During the vigils I spent the time for the most part engaged in ordinary occupations, reading, marking essays, knitting, working out results of experiments; anything of a mathematical nature kept me awake better than light reading. I found it advisable to make out a programme for each hour, of such a nature that by working hard I could complete the set task: otherwise there was a tendency to fall into a light doze or a state of such acquiescence in mere existence as hardly to warrant being called awake.

physiological or mental, which did not affect me for more than one day, *e.g.* slight headache or worry.

It is always necessary when judging any particular day's performance to take into account not only that day's record but the weekly average: a high average deviation is invariably significant of some nervous instability, or of unusual conditions, *e.g.* in the above graph the two days when the records are worse than normal there had been slight excitement.



GRAPH 2. 1st fatigue cycle showing the effect of three nights of diminished sleep and the gradual return to normal interfered with by further vigils. The dotted lines represent the extremes of normal variation. June, 1912.

* = effect of 1st vigil, ** = effect of 2nd vigil, *** = effect of 3rd vigil.

Note the effect of further lack of sleep in bringing back the errors from nearly 200 to 91.

As a result of three nights with little sleep the immediate effects are either normal records or records showing marked diminution in the number of errors, *e.g.* during the first cycle the record after the third night is fifty-three errors, the lowest number I had ever attained: as soon, however, as I made up for the loss of sleep by extra sleep on succeeding days the errors rose to over 200 and continued to range between 110 and 202 for sixteen days, at which stage the return to normal was interfered with by another experiment. I have pooled all the errors, as the improvement at first and deterioration afterwards affect equally all types. See Graph 2.

The loss of control as evidenced by the inability to restrain the hand from making unaimed extra dots is very marked and is as characteristic of the fatigue state as the loss of efficiency shewn by the ineffective aiming.

In all the other sets of experiments with the dotting machine, and there are five under exactly the same conditions, extending over a period of three years, there are similar results, viz. improved or normal records at first, followed by a period of slow and irregular return to normal, the normal being reached about the sixteenth day[1].

The subjective records, written in all cases before knowing the result of the dotting, are also instructive. During the rather prolonged period of recuperation the impression that the dotting was good was very common, *e.g.* on a day when actually there are 202 errors the note runs, "am quite sure I am in good form; thought the dotting very good[2]." Under ordinary normal conditions I rarely speculated about the success or otherwise of the work[3].

The Effect of Fatigue on the Associated Words.

Under normal conditions the average number of errors in a list of 40 words was 5 (extremes 4 and 7); the immediate effect of three vigils is to reduce the average to 3, followed by such records as 12, 13, 10, 16, 26, 18 and so on until on the sixteenth day the normal of 6½ errors is reached.

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[1] The record on the day following the first vigil I called the first day and so on.

[2] This particular delusion occurred so frequently that it became a subjective index of the reverse.

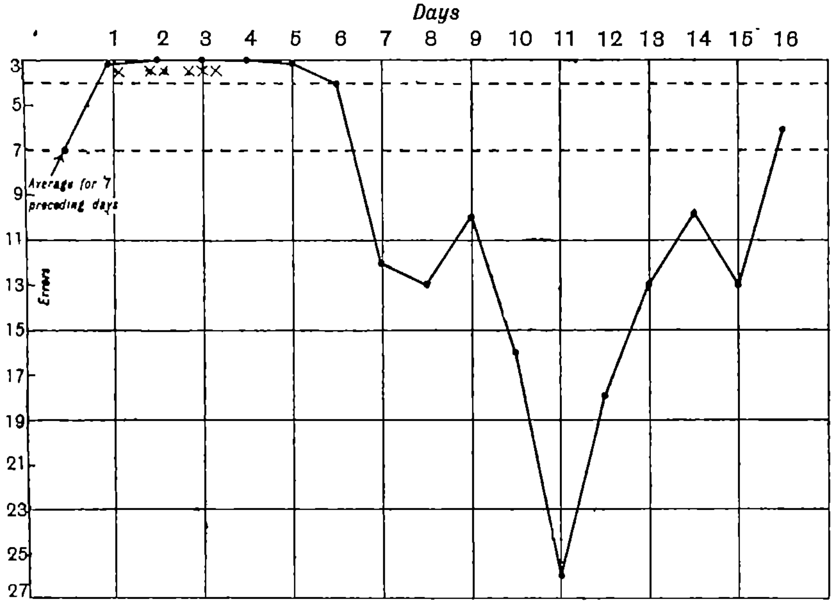
[3] At another time I got the effect of one night with one and a half hours' sleep. The result is analogous, viz. to improve the dotting on the following day, to give a normal record on the succeeding day and to increase considerably the errors for the next three days; on the sixth day there is a return to normal.

In two 'cycles' there are complete records of the words and they are similar in character. I have illustrated the later one, as the return to normal was not complicated by other experiments.

Such lists involve two quite distinct factors:

(1) Power of attention to note each connection,

(2) Ability to reproduce the connections that have been seen. Under the second phase of fatigue there is conspicuous failure in both, *e.g.* sometimes certain sequences were not consciously attended to; at other times it was recognized that the connections had been appreciated at the time but could not be recalled[1].



GRAPH 3. Showing the effect of fatigue on the associated words—daily variations. Normal variations fall between the dotted lines. July, 1915.

The Effect of Fatigue on the Windmill Illusion.

During one fatigue cycle almost daily records were obtained with the windmill illusion. Under ordinary circumstances, and assuming the attitude of trying to maintain the windmill revolving in one direction as long as possible, in my own case I got on an average seven changes a minute. The immediate effect of fatigue was to increase the number of changes to between thirteen and forty for several days[1].

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[1] That these two factors can be isolated is well brought out in the Effect of Alcohol, details of which will be published later.

The Effect of Fatigue on the Learning and Re-learning of Nonsense Syllables.

This test, which is of the nature of a motor habit[2], was only used during one cycle of fatigue experiments. It consisted of learning twelve nonsense syllables, noting the number of repetitions required before they could be reproduced, and then of re-learning them 24 hours afterwards. Although the effects of fatigue are not as well marked as in the other tests yet they are not in conflict with the others. The learning process remains normal during the first phase, characterised in the other tests by improvement, and then falls off for a period about equal in duration to the second phase of the dotting. The re-learning is equally adversely affected. Normally I require seven repetitions to learn a group of 12 nonsense syllables and three to re-learn after 24 hours. The effect of fatigue after the first phase was to increase the number of repetitions for learning to an average of thirteen and for re-learning to an average of seven.

It is to be expected that a process involving habit[3] should not be as seriously affected as one of the higher processes; there is also the difficulty that it is not easy to maintain the same attitude throughout the learning, the tendency being to fall into a condition of relative passivity during some stages of the fatigue process, the test not being exciting enough to demand the greatest possible effort.

The Effect of Fatigue on Speed of Tapping.

In this test, too, the effects are not as striking as in the case of the dotting and words, but they are significant. The first phase of strain gives normal and abnormally good results followed by a gradual deterioration in the number of taps made in one minute. Unfortunately it was not possible to complete the records until a normal was regained, nor were daily records obtainable[1].

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[1] Little importance can be attached to the later figures as unfortunately for the purpose it is difficult to rule out suggestions, *e.g.* knowing after a few days' experience that the number of phases per minute had increased I could not be certain that I was as successful in controlling the direction as before. The immediate results quoted above are quite reliable as I had no preconceived theory on the subject and did not know what to expect.

[2] Cp. Bergson, *Matière et Mémoire*, p. 75.

[3] The correlation between the learning of nonsense syllables and pure motor habit has been found to be ·6. Details of these experiments will be published later.

The Effect of loss of Sleep on the Dotting and Lists of Associated Words Combined.

In order to test the reality of the improvement characteristic of the first phase, experiments were made on the effects of the same loss of sleep on the dotting and words combined, *i.e.* instead of performing the two operations of dotting and reproducing lists of words at different times, a list of fifty words was read through twice by a collaborator while the writer dotted the usual length of tape under the usual conditions. Read at the rate of one word in two seconds, the fifty words read twice lasted the same time as the dotting. At the end, the list was reproduced and errors counted in the usual way. In this case the problem was to perform together two operations both demanding concentrated attention. Before using it as a fatigue test I had had almost daily practice in performing the combined operation for seven weeks so that the improvement due merely to practice was well passed. The average number of errors in dotting for three weeks immediately preceding the fatigue period was seventy-four and for the words twelve (extremes eight and sixteen). As a result of one night with one and a half hours' sleep the errors in dotting fall to sixty, and the words to four, followed by similar records for several days. The improvement resulting from marked fatigue is thus clearly shown[2]. See Graph 4.

On the 5th day the dotting and the words show deterioration, 107 errors in dotting and ten errors in words. On the 12th day there is a return to normal of both; but whether this would have been

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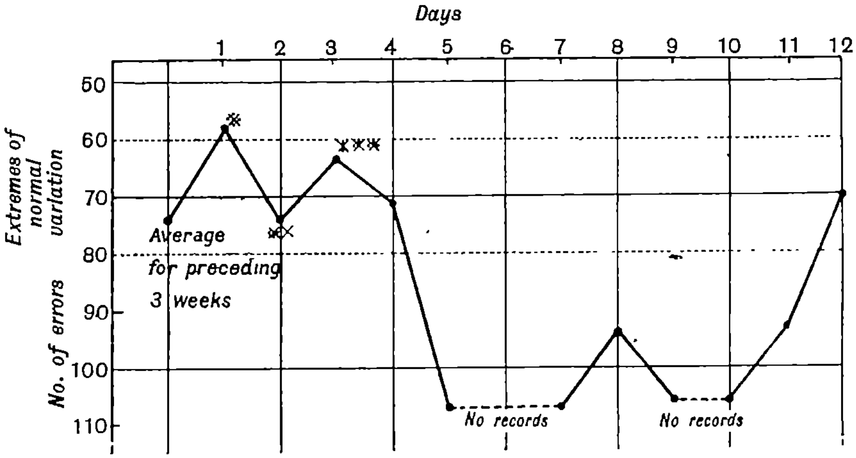
[1] Another subject undertook to cut down her usual amount of sleep to one-third for three nights. The tests used in her case were:

(*a*) A slightly modified form of the dotting, only the alternate circles being marked.

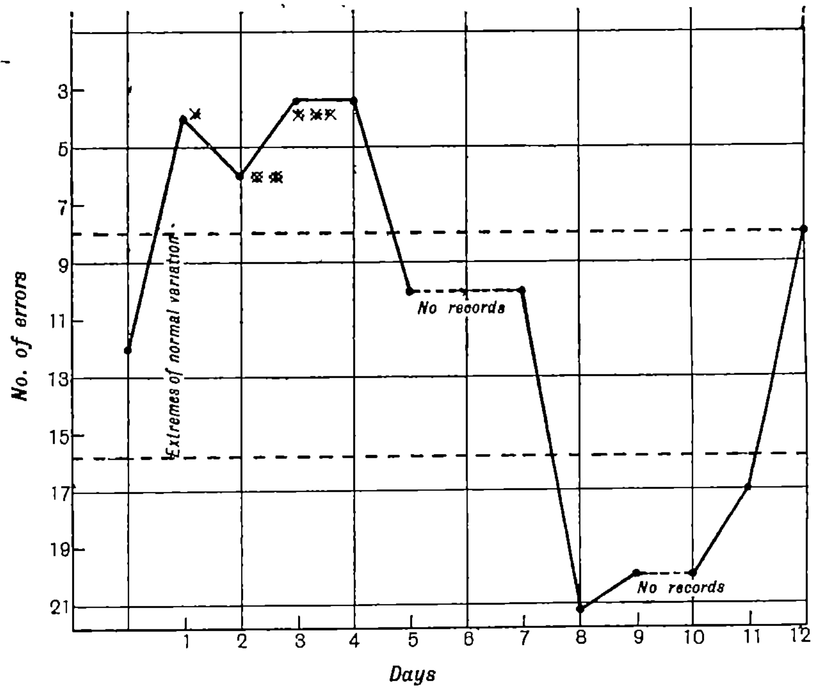
(*b*) A list of 100 words, as described above, read twice and reproduced immediately afterwards.

Unfortunately daily records were not available, but during the period of cutting down sleep the records obtained of the dotting are either normal or unusually good, *i.e.* between 80 and 100 errors, and for six days from the beginning of the experiment there are no bad records: on the 7th day they rise to 159 and the average for that week is 128 and for the following week 135. With the words the average number of errors under ordinary conditions was 23 (extremes 18 and 35). During the period of stopping up the number is normal, viz. average 27, but the errors rise to 43, culminating nine days after in 71, after which there is a gradual fall to 20, sixteen days from the beginning of the experiment.

[2] Experiments to be published later show that there are abnormal conditions where one task improves at the expense of the other This did occasionally also occur under apparently normal conditions, but only rarely.



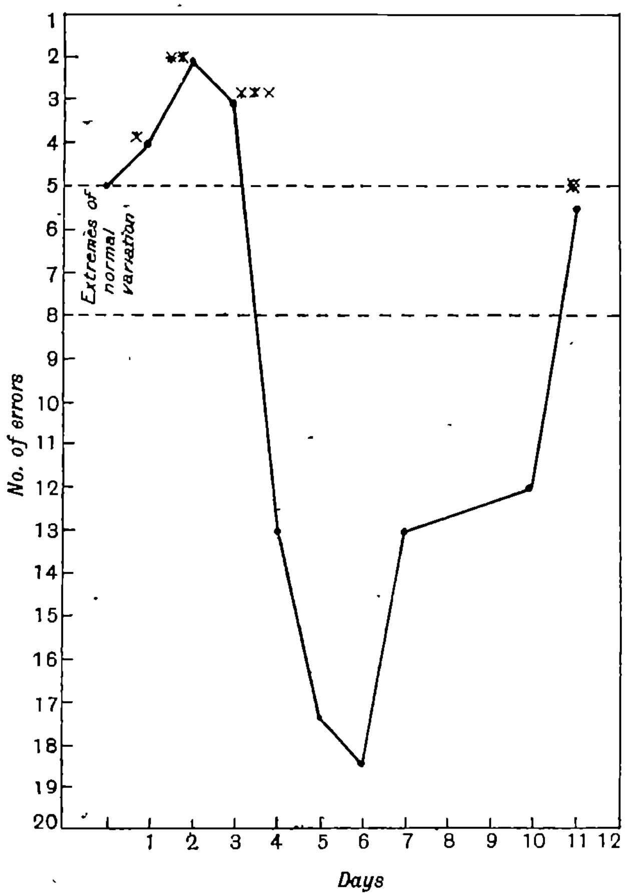
Fatigue effects shown by errors in 'dotting' when combined with 'words.'



GRAPH 4. Errors in 'words' when combined with 'dotting.'

maintained cannot now be known as incipient influenza made all the following week's records extremely variable[1].

(This test involving the double task might be valuable as a means of estimating the effect of distraction.)



GRAPH 5. Effect of loss of sleep on associated words. Also effect of further loss of sleep on a state of fatigue.

*=effect of 1½ hours' sleep, **=effect of 3½ hours' sleep, ***=effect of 5½ hours' sleep.

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[1] Note on Graph 4. I only give this graph to show the effect of fatigue on the double test and it is only of value for the first phase as the second phase was complicated towards the end by the effects of a severe cold.

Effect of Fatigue on a Fatigued State.

So far we have only considered the effect of fatigue on a normal condition; the effect of fatigue on an already fatigued state is also important.

To show this there are in this research two sets of experiments:

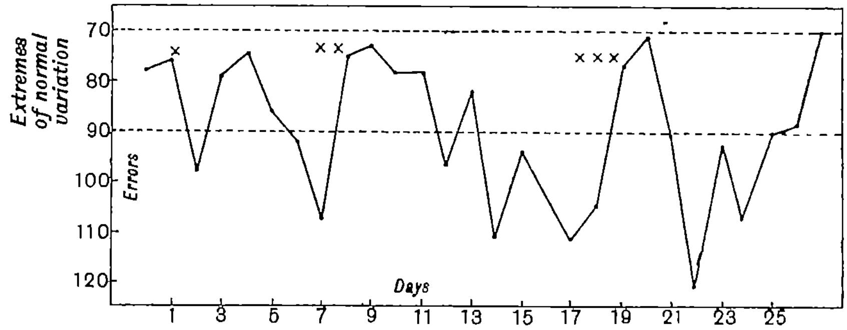
1. The effect of a night of one and a half hours' sleep when the influence of the three nights with curtailed sleep was still apparent. (See Graph 1 and Graph 5.)

2. The effect of the same loss of sleep in so far as the actual number of hours were concerned, but with the vigils separated by intervals of several days instead of being in immediate succession.

The first type is illustrated in Graph 5 on the preceding page, which shows the effect on the associated words, and in Graph 2 (page 333) which shows the effect on the dotting test.

It will be seen that the immediate effect is to bring back the errors to a good normal at a time when the deteriorating influence of fatigue is well marked, *i.e.* from 12 to 5½ in the case of the words and from 200 to 91 in the case of the dotting. The other records show that a normal result would not have occurred at this stage without the intervention of some other factor.

The second type is illustrated thus:



Graph 6. Showing the effect on the dotting of the same loss of sleep at intervals. July 1914.

*=effect of 1½ hours' sleep, **=effect of 3½ hours' sleep, ***=effect of 5½ hours' sleep.

The method of procedure was as follows. I stayed awake the first night as in the previous experiments; after an interval of seven days, by which time the second phase was in evidence, I stayed up part of another night for a time equal to the second night of previous experiments; after a further interval of ten days, which again allowed for the full sway of the second phase, I stayed up part of another night equal to the third night of previous experiments.

The object of this was to find out how long it took to return to a normal condition as compared with the three nights in immediate succession. The results are all quite clear. Each night involving lack of the normal amount of sleep affects the work advantageously, *i.e.* brings the errors back to normal, but it is not until twenty-five days from the beginning of the experiment that normal records are established; the preceding and succeeding fatigue cycles, the vigils being in immediate succession, give a return in sixteen days. It will however be noticed that as a balance to this the errors are not as great.

The deleterious results of fatigue on an already fatigued state are thus exhibited. Such a condition is somewhat analogous to a state of chronic fatigue, *i.e.* continued work on an already fatigued state.

COMPARISON OF THE SEVERAL FATIGUE CYCLES.

If now instead of considering each experiment in isolation we compare five sets which were done under the same conditions as regards loss of sleep and manner of spending the vigils, some striking differences reveal themselves:

1. There is a delay in the onset of the second phase, *e.g.* on the first occasion, June, 1912, this phase set in on the 4th day, *i.e.* immediately the strain was over: on the last occasion, July, 1915, it is not until the 7th day that this phase begins and the intervening experiments illustrate the gradual extension of the first phase.

2. The return to normal is hastened, *i.e.* the duration of the second phase is shortened, *e.g.* in the June, 1912, cycle there was not even an approximation to the normal by the 16th day, whereas in July, 1915, a reliable normal was attained by the 16th day. Full comparisons of the intervening ones cannot be made owing to complications of further vigils to get the effect of fatigue on a fatigued state.

3. If we compare the improvement due to loss of sleep in the June, 1912, series with that of July, 1915, we find that the improvement becomes less marked, *i.e.* the percentage improvement is at first 26 as against 13 in the final experiment. I regret that the lists of words were not used earlier; during only two sets of vigils are they comparable, but during the first time of using them the average number of errors due to the first phase of fatigue is 2·8, on the next vigil, one year later, the number is 3·3. The difference is extremely slight, but considering the reliability of the errors of the test under normal conditions it is

significant as to direction: also in the first cycle there is one occasion of only one error and one of two errors, whereas in the last set the lowest number of errors is three.

It may be that there is here some evidence indicating the possibility of setting up a relative immunity to a particular form of fatigue. Is there produced naturally the antitoxin to fatigue whereby the individual is rendered more and more immune? The fact that in the last series there is a less exaggerated effect of the stimulating actions of fatigue looks as if some neutralising agent were at work preventing the full effect of the fatigue products and helping to conserve the potential forces.

My figures are really only suggestive and many more experiments are needed for proof, but the possibility of such proof is a fascinating prospect. Some biochemical psychologist may one day be able to analyse the fatigue toxin and supply the requisite antitoxin[1]! There is little doubt that people vary considerably with regard to fatiguability, both for specific tasks and for continuous labour. Are some of the famous workers people whose organism protects itself not only by giving premonitory warnings in various subjective fatigue symptoms but also by naturally inoculating itself against the toxins of fatigue[2]?

SUBJECTIVE EFFECTS OF FATIGUE.

It is hardly necessary to draw up a list of the various symptoms by which fatigue reveals itself to the individual. It is, however, pertinent to analyse these symptoms carefully, and this analysis will convince the observer that even subjectively the state of fatigue reveals itself as complex, complex not as a mere accumulation of fatigue sensations from various organs, but complex in the sense of being compounded of diverse opposite symptoms and emotional attitudes.

The first phase would, if it were to be described in terms of pleasure-pain, distinctly involve both, *e.g.* the muscular weariness due to the strain of continuous work is particularly noticeable in the heaviness of the eyelids, and the feeling of general tiredness, becoming at times positive pain, sufficiently intense to intrude into the focus of attention spontaneously: mentally, however, the feeling of exaltation, combined with an emotional belief in the power to conquer all things, is distinctly pleasurable. In my own case this double feeling tone is most marked, even though for the most part the pleasurable aspect is dominant.

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[1] Hitherto the experiments along these lines have been confined to local muscular fatigue.

[2] Cp. Cannon, *The Bodily Changes in Pain, Hunger, Fear and Rage*. The effect of adrenalin on fatigue is described.

Another complexity of the fatigue state in its first phase is the feeling of inertia showing itself in a disinclination to begin any work, but when once this inertia is overcome by some stimulus, objective or subjective, work progresses easily and with the feeling that there are unlimited stores of energy available: one's machine, if one may so liken the body, seems to be in excellent working order.

In this state too, hyper-sensitivity is also marked, so that noises to which one is normally indifferent become painful, while the general excitability makes one easily affected to 'smiles or tears[1].' Becoming marked about the third day is a tendency to 'perseveration' so that it is difficult to maintain an interest in continuous unstimulating work owing to the intrusive force of any idea, which, until it receives expression, remains an irritating obsession, *e.g.* suppose I am reading a fairly stiff book, a word in it may remind me of some trifling task that must be done sometime, I find I must get up and do it; I then return to my book and the same process goes on. Normally I should make a note of the task and pursue my reading.

During the second vigil of the first cycle I experienced strongly marked visual hallucinations, viz. opaline spheres, semi-transparent bodies and gossamer threads, sparkles of light at intervals on the dark background of my desk. In none of the later experiments did I get such phenomena. All these symptoms rapidly passed away, none of them surviving the first phase, while, except for a slight headache, and a general lowering of the *joie de vivre* which usually heralded the second phase, a few days of extra sleep soon dissipated any symptoms and apparent normality ensued. Subjectively I had no criterion whatever for the stage when the objective records showed continuous deterioration nor yet for the return to a normal, and yet that the dotting machine and words are not alone in exhibiting this return is shown by the different reaction towards alcohol, which plainly shows two phases even in what is here called the second phase[2].

THEORETICAL CONSIDERATIONS.

The evidence afforded by these experiments opens up several interesting theoretical problems.

If the results are not merely chimerical, then instead of defining fatigue in terms of diminished output, we shall have to modify the definition by the addition of 'ultimately' and let fatigue be the state which *ultimately* results in a diminished capacity for work, since we have seen that the immediate effect is to improve the capacity for work: or if we cling to the ordinary definition then the first phase of the fatigue process must be designated by some name other than fatigue and fatigue kept for the second phase, as seems to be Mosso's idea when he says "in many persons extreme fatigue is preceded by a phase of excitation which may last for a long time[1]."

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[1] Cf. James, *Principles of Psychology*, II. 539.

[2] Details to be published later.

Physiological treatises discussing the products of fatigue are agreed as to the complex nature of the substance produced: now if we suppose that the final result of the fatigue toxins is to produce a substance allied to the drugs with a stimulating action, then we should get that marked improvement so characteristic of fatigue, whether that fatigue operates on a nervous system already fatigued or unfatigued. Such a result could be accounted for if we assume that the action of some of the fatigue products is to make the energy of the nervous system more accessible, *e.g.* the output of a machine might be increased by the addition of more fuel, or by making the parts work together with greater ease. It is hardly a rational hypothesis that fatigue actually supplies the organism with more energy from without, but if the natural resistance at the synapses[2] were by some means lessened, so that the potential energies of the nervous system were set free, this might account for some of the facts: if too we postulate the 'drainage' theory as expounded by McDougall[3], then the particular channels which happened to be directly stimulated would drain more easily the energy of the neighbouring channels and hence the superior output. The ease and fulness with which in the associated words relations are perceived and reproduced in the first phase of the fatigue state seem to be thus accounted for, and also the subjective feeling of a very tremulous success and hyper-sensitivity so characteristic of the earlier phase: in my own case I found that the maintenance of concentrated attention on an unstimulating task was more difficult and that there had to be a regular reinforcement of the particular problem by the appeal to subjective motives, owing to the intrusive force of any image or idea that happened to be called up. The general state can be designated as one of unstable equilibrium. It is probably akin to those conditions which in some

[1] Mosso, *Fatigue*, p. 236, Eng. Trans.

[2] Cp. Sherrington, *Integrative Action of the Nervous System.*

[3] (*a*) *Physiological Psychology*, p. 37. (*b*) *Brain*, XXVI. ii.

people herald a nervous breakdown, viz. an uncontrolled feverish energy which temporarily is extremely effective but which ultimately leads to disaster. In the case of these experiments the stimulation is temporary, a question of hours, not of weeks or months, and the required rest speedily removes this condition of hyper-excitement. But would one not expect that the return to a normal condition would ensue as soon as the fatigue products had been removed or neutralized. If when a drug effect is apparent another with an opposite action is taken, the resultant normality, in so far as my experience of a very few experiments goes, is final; in fatigue we do not get a speedy return to normal, but on the contrary an exaggerated opposite effect. Evidently the chemical explanation is not the only one. If the subjective symptoms, which appear as soon as the ordinary time for cessation of work looms, were indexes of the exhaustion and not rather premonitory indications —sign-posts as it were that along that path lies fatigue— the individual would be unable to continue his work. Force of will however can conquer, or at least refuse to be deterred by, these indications, and persist in driving the victim along the dangerous path. This, involving as it does the expenditure of considerable nervous energy, which however is not being made up for by adequate rest, necessitates the using up of the stores of potential energy during the first phase which is rendered dangerously easy by the effects of the fatigue products in lowering resistance. It is as if a man who has habitually lived on his income is suddenly confronted with an unforeseen demand for money. He may, as a solution, break into his capital, *i.e.* he will temporarily have command of greater resources than normally are his, but his reserve force is thereby lessened so that he has later less at his command[1].

In the article referred to, McDougall suggests that the function of the resistances is essentially the limitation of activity so that they are protective of the energy of the organism. This seems to be true in all those numerous cases where continuance of work results in a fairly regularly diminishing activity, that is when there is no over-strain.

If, however, the strain is greater than the resistance is normally adjusted to, the protective action cannot operate, and the individual is enabled to work at his will to exhaustion point, so that we get an example of McDougall's second point, viz. that "Fatigue is relative, *i.e.* the expression of the rise of the ratio between resistances and energies, so that if the conditions are such as effectively to call into play all the great special sources of energy this ratio may be kept from rising above the normal until the whole organism approaches absolute exhaustion."

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[1] Cp. McDougall, "The Conditions of Fatigue in the Nervous System," *Brain*, 1909.

Did the synaptic resistance always operate in the same way, we should have an individual incapable of adjusting himself to any excessive demand and hence useless in emergencies. It is at least possible that there are people who are constitutionally better protected, from one point of view, than others in this respect, people farthest removed from the hysterical or neurasthenic type: should this resistance be excessive we should get a type incapable of meeting the demands of a strain; there would be no fear of such exhausting their reserve forces, but there might be a paralysis of action and thought altogether.

But this point of view tends to treat the individual as if when he had returned to normal he was as he had been before the strain. When, however, we treat him as a person of continuous existence, each fatigue cycle must be considered as having its specific effect and rendering the individual different in his reaction for the future, *i.e.* in these experiments, towards the next fatigue cycle. Weichardt[1] maintains that the antitoxin to fatigue is produced during normal functioning activity so that with the appearance of moderate quantities of the products of fatigue there always occurs in the healthy organism an increased formation of the specific antitoxin. If this be so, the ease of the expert as contrasted with the excessive labour of the novice, may be a special case of the formation of the antitoxin.

As mentioned above, a comparison of the several fatigue cycles certainly suggests the setting up of a comparative immunity. Naturally it will only be relative, as immunity to any other disease can be overcome as a result of the circumstances favourable to infection[2].

If fatigue can be treated as a disease with its corresponding antitoxin, then from a practical point of view fatigue within certain limits is a positive advantage to the organism, leaving it not with lowered vitality but with added resistance. Such a thesis, however, could only be maintained for fatigue which was not so excessive as to deplete the very sources of vitality.

SOME PRACTICAL CONSIDERATIONS.

The problems of fatigue are becoming more and more insistent in all departments of human life, industrial, educational and medical. The exceeding extravagance of exacting further work from those who are already under the influence of previous fatigue is not generally realized: even in the case of the very small excess of work, such as the experiments recorded above demonstrate, there is, in an unmistakable form, evidence of the unprofitableness of the transaction. The possibility of the second phase of fatigue extending far beyond the subjective indications is of urgent importance industrially. It has been shown that fatigue involves at least, loss of accuracy, failure of memory, lowering of speed; where such inefficiency, instead of displaying itself in the secrecy of the laboratory, imperils the life of the worker himself or of others, the problem becomes momentous. It is highly probable that a connection might not be realized between fatigue and an accident, if the latter occurred considerably after the fatiguing experience, and yet it is just when the strain ceases to be felt, and the vigilance therefore is relaxed, that the greatest danger occurs. It may be possible some day to discover a means whereby those employed in dangerous trades will be tested regularly as to their fitness, and as stringent regulations made for securing them adequate rest as are made for nurses. What is oftentime called carelessness might well be fatigue manifesting itself in temporary forgetfulness, *e.g.* as in the above memory experiments. Such forgetfulness is unfortunately only too important sometimes, for instance in the case of railway signalmen. Again the oft-expressed surprise that accidents do not increase in number at the end of a given work period, may, apart from various other possibilities, be due to the stimulating effect of excessive fatigue[1].

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[1] Burnham, "Problems of Fatigue," *Amer. J. of Psychol.* XIX.

[2] Steinberg, *Infection and Immunity*, 1903.

Taking into account the extravagance of working on a fatigued state, the possibility of chronic fatigue should be forestalled and due provision made for adequate rest.

The various types of work should be analysed from the point of view of their demands on the human organism and the hours of work adjusted accordingly. To fix an eight hours' day for all branches of all trades would not necessarily prove beneficial: four hours of some work would be more fatiguing than ten hours of another type. And not only must the work be analysed, but the fatiguability of the individual determined so that to each man is assigned that work for which he is best adjusted.

That we are far from being in a position to dogmatize on either aspect is only too true, but the field of work is well worth cultivating.

While recognizing however, that the human organism cannot be subjected to fatigue with impunity, nevertheless we must take into account that there will be a gradual adjustment to the conditions of the work, *i.e.* a relative immunity to fatigue, so that one cannot advocate as a universal law the avoidance altogether of even excessive fatigue: it may well be that it is an advantage to be trained in resisting fatigue. And there may be occasions when a man or woman ought to be able to give of his or her reserve forces: the psychologist can but point out that there are results accruing from such action which ought to be taken into consideration, and that due precautions should be taken.

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[1] The relaxing of the strain through the knowledge of the imminent cessation of work, or the extra care in order to avoid catastrophe just before stopping, will also play a part.

Again, the unshakable belief of most students that to work up to the last moment before an examination is a profitable proceeding may perhaps find its justification in the stimulating nature of over-fatigue. If the examination be of short duration the candidate may manage to get it all within the compass of the first phase; a prolonged ordeal would certainly prove the futility of the lack of rest. Perhaps, too, many of the nervous breakdowns attributed to higher education are due to loss of sleep no less than to the strain of learning, students not being as a rule addicted to early hours.

GENERAL SUMMARY.

Within the limits of the evidence afforded by these experiments, the following generalizations seem valid:

1. Fatigue as estimated objectively involves two distinct phases:

(*a*) A phase when fatigue acts apparently as a stimulant, so that work demanding concentrated attention is done more effectively than under normal conditions[1].

(*b*) A phase of longer duration when the body is attempting to make good its losses—which phase is characterized by a general loss of accuracy of aiming, in a weakening of the powers of inhibition, as shown by the increase in the number of uncontrolled dots, in marked loss of the power of concentration, as shown by the inability to attend to the words of a list, and in loss of retentiveness, as instanced by the inability to reproduce the words when the connections have been realized, and in the increased number of repetitions required to re-learn a group of nonsense syllables.

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[1] That these apparent improvements are not due to the effect of habit is shown by the fact that the normal records obtained after the whole fatigue cycle was over do not show any deviation from the preceding normals.

2. The subjective feelings bear no relation to the objective demonstration of fatigue, extremely bad work being not infrequently accompanied by a conviction that it is unusually good.

3. There is the suggestion that it is possible to become partially immune to a particular form of fatigue.

4. The time taken to return to a normal condition after the loss of but a few hours' sleep is disproportionately great; and this return is gradual but irregular.

5. Fatigue acts on a fatigued state as it does on the normal, *i.e.* while the strain is present there is improvement, but the return to a reliable normal is considerably delayed.

As a result of the experiments it would appear as if both the dotting machine and the list of associated words were useful instruments to measure fatigue: tapping and nonsense syllables are not refined enough, as such processes will not be affected as quickly as the later developed and so lesser organized processes, such as voluntary attention and rational memory. Of the two the dotting machine is probably the better as it is impossible to insure equality of interest and difficulty with the words; also in the case of the machine there is no possibility of influencing it: the individual must adjust himself at each moment to the machine, he cannot make up by an extra spurt for spasmodic lapses, since the unmarked circles or indifferent aiming are there as records against him. It is temporarily as inexorable as 'the moving finger' of time.

In ordinary work which is under the control of the individual much of it is probably done by 'spurts' and 'rests.' The familiar falling off of 'output' in the afternoon may be due in some cases to a voluntary relaxation after the morning's effort: in such ways the body is protected against the effects of excessive work.

The windmill illusion is more interesting theoretically than practically from the point of view of these experiments.

Note on the Dotting Machine.

As the work of getting daily dotting records progressed, it became evident that the machine was a very subtle indicator of well-being, frequently foreshadowing some physiological change. A not infrequent experience was to find a bad record on a certain day allied with the subjective statement of 'good form,' followed, though on the next day, by the remark 'have a headache.' This could not be due merely to suggestion, for it happened usually that the tapes accumulated for marking, so that I was unaware of the number of errors on a given day.

Again, a week in which there was a considerable average deviation was significant of a departure from the normal health, *e.g.* the mean variation, normally 7, rose to 13, the results alternating between very good and very bad records.

Since completing these experiments I have used the dotting machine as a test of fatigue on a number of girls engaged in clerical work, and get results quite in harmony with those given above.

It also has proved in a few cases, which happened to come to my notice, to be an indicator of nervous disorders. Care must however be taken in interpreting results. It is not merely the number of errors in a given length of tape that is significant, *e.g.* two people in a length of three metres might get 100 errors: suppose *A*'s errors are fairly evenly distributed throughout the length and that the mean variation is small as estimated for 6 half-metres, the figure may reasonably be taken as representing her capacity for such a task: suppose *B*, on the contrary, does one metre almost without error and then suddenly breaks down and either refuses to go on or makes shots wide of the mark unaimed and uncontrolled, then it would be well to make enquiries as to *B*'s physical condition.

As with myself I found that the subjective feeling of success or failure was quite unreliable. A safer subjective criterion was the statement, which some subjects made after a record which proved to have a large number of errors, that the machine was going too quickly. Whenever a subject asked if the pace of the machine had been increased there was invariably a bad record.

The interest aroused by the test is sufficiently great to permit of a wide application. Records from school children carried on over a sufficiently lengthy period might throw some light on the problem of the fatigue engendered by the various school subjects, and also of the accumulative effect of a term's work.

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THE INFLUENCE OF THE FORM OF A QUESTION.

By BERNARD MUSCIO.

(*From the Cambridge Psychological Laboratory.*)

1. *Aim of the experiment.*
2. *Introduction.*
3. *The question forms investigated in this experiment.*
4. *Description of the experiment.*
    (*a*) *Observation material and apparatus.* (*b*) *Subjects.* (*c*) *Procedure.* (*d*) *Character of the questionnaire.*
5. *The individual questions and their answers.*
6. *Exclusion of certain of the original questions.*
7. *Analysis of answers for each question form*
8. *Plan of discussion of results.*
9. *Subjective-direction compared generally with objective-direction question forms in respect of* (*A*) *Caution,* (*B*) *Suggestiveness,* (*C*) *Reliability.*
10. *The specific question forms compared.*
11. *Conclusions concerning the specific question forms.*
12. *Practical value of conclusions.*

1. AIM OF THE EXPERIMENT.

IN this paper is presented an account of part of an experiment dealing with evidence. The specific aim of the part here described was to compare certain question forms in regard to their respective influence upon answers, it being assumed, as a result of previous experiments in the psychology of evidence, that the answer to a question is under certain conditions partly determined by the question's form. Its more general aim was to obtain data from which an analysis of the various mental processes operative in determining answers to questions might be made.

2. INTRODUCTION.

Binet[1] considered that *whenever* a question was put to a witness his memory was 'forced.' He distinguished, however, between different degrees of 'forcing.' The type of certain questions rendered them, in his opinion, moderately suggestive, while other questions, for a similar reason, were strongly suggestive. The type of question considered by Binet to be moderately suggestive is fairly unambiguous. It is the question that contains a negative: "Isn't the button fastened to the card with thread?" There is difficulty, however, in discovering what types of question Binet meant by the unsuggestive and the strongly suggestive questions. His list of unsuggestive questions[2] contains questions of types which have since been distinguished as different. Thus, it contains the two questions: "What colour is the portrait?" and, "Is the stamp new or does it bear a post-mark?" Formally, these two questions are very different. The latter is now distinguished as a 'complete disjunction' question. Similarly, his list of strongly suggestive questions[3] contains questions of different types. For example, it contains the two questions: "At what part (of the picture) is the little dog?" and, "Has the gentleman the left leg crossed over the right one, or the right one crossed over the left one?" These questions again are formally distinct in type, the latter being now designated an 'incomplete disjunction' question.

The fact is that Binet used no one principle of classification in distinguishing different types of questions, but based his distinctions partly on the question form and partly on the question content. Thus, the moderately suggestive question always contains an identical formal element, the negative, and is always concerned with something that did not occur in the observed material. The unsuggestive questions, so far as they are of any one formal type, are chiefly complete disjunction questions (with regard to which one of the alternatives offered *must* be true). Similarly, the strongly suggestive questions, so far as they are of one formal type, are chiefly incomplete disjunction questions; but perhaps the principal element which renders them strongly suggestive is their content. They are sometimes, *e.g.*, preceded by a false statement concerning the observed material. The result is that Binet's work, so far as the relative importance of different question forms is concerned, is of value chiefly because of its suggestiveness.

Since the publication of Binet's experiments, a number of investigations of a similar general character have been carried out in Germany.

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[1] *La Suggestibilité*, ch. VI. [2] *Ibid.* p. 297. [3] *Ibid.* pp. 299–300.

As a consequence, Binet's rough and ready classification of types of questions has been much improved upon, although there exists even yet no complete classification. Stern was the pioneer of the German work on evidence. He distinguished at least six types of question. A more recent investigator, Lipmann[1], distinguishes seven types, as follow:

(1) *The determinative question forms* ("The question with the question word"); *e.g.* What colour is the shirt?

(2) *The complete disjunction question form; e.g.* Is the child in the meadow asleep or awake?

(3) *The Yes-No question form; e.g.* Is there a cupboard in the room?

(4) *The No question form; e.g.* Might the man have a spoon in his hand?

(5) *The Yes question form; e.g.* Isn't there a cupboard in the room?

(6) *The incomplete disjunction question form; e.g.* Is the shirt red or green?

(7) *The implicative question form* (questions with false implications); *e.g.* What is the child in the meadow doing? (This implies (1) that there is a meadow, (2) that there is a child in it.)

It will be noticed that this classification is not made on any one principle; *e.g.*, most of the question types, whatever else they may be, are '*implicative.*' "Is the child in the meadow asleep or awake?" (the complete disjunction question), is quite as implicative as "What is the child in the meadow doing?" In connexion with the present experiment, however, no attempt has been made to elaborate any complete classification of question forms. For a chief aim of the experiment was to investigate a distinction not hitherto noticed. Since previous classifications have not been made on any one principle, the possibility of their being complete was a mere matter of chance. And, in fact, they have *not* been complete. One fairly obvious and very important distinction between types of questions has been omitted from them.

This distinction may be illustrated in the following manner. Consider the two questions: "Did you see a pistol on the table?" and, "Was there a pistol on the table?" Outside a psychological laboratory, the aim of anyone who might ask either of these questions would be to obtain information as to *the presence of a pistol on the table.* The former question, however, asks if the pistol *was seen*, the latter if it *was there.*

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[1] *Ztsch. f. angew. Psychol.*, July, 1907 (*Die Wirkung von Suggestivfragen*).

It is not possible, strictly speaking, to say that the two questions differ in form only, for literally they differ also in content, even if the 'was there' be interpreted as Berkeley would have interpreted it. The relation between the contents of the two questions is nevertheless very close. Both contain as an identical element 'pistol on the table.' Further, when both questions are asked with regard to observed material, they *may* be considered to have an identical content, since 'was there' *may* then be taken as equivalent to 'Did you notice?' It is not necessary, however, to adopt any one-sided attitude here. The two questions may be asked, and the aim in asking each may be identical, namely, to obtain information about *the presence of a pistol on the table.* In the present experiment questions of these two forms (or types) are compared *from this point of view,* that is, with regard to the character of the information yielded in answer to each so far as this information concerns 'objective fact.'

In other questions of the form "Did you *see*...?," the specific term 'see' may be replaced by other specific terms, such as 'hear,' according to the nature of the material observed; or all specific terms may be replaced by such generic terms as 'perceive' or 'notice.'

I propose calling all questions of type similar to that of the question "*Did you see* a pistol on the table?" *subjective-direction*[1] questions, since they tend primarily to direct the attention of the person who is questioned to his actual prior *relation to* the object; and for an analogous reason, I propose calling all questions of type similar to "*Was there* a pistol on the table?" (or, *e.g.*, Did B. whistle?), *objective-direction*[1] questions, since they tend primarily to direct a witness's attention to the *object observed,* and *not* to his observation of it. Every question is either subjective-directive or objective-directive[2].

The importance of the above-stated distinction depends upon the necessity, which sometimes arises, for using *leading* questions. Jurists allow leading questions to be employed in certain circumstances; *e.g.*, if a witness is hostile, or if the matter of examination is very complex (when it is assumed that a leading question will 'refresh' memory). Although a leading question is never exactly defined, it would generally be agreed that some leading questions are more misleading than are others. Thus, it would probably be agreed generally that to ask whether the coat was very tattered is more suggestive than to ask simply if it was old. Instances shewing an even more obvious difference in degree of suggestiveness could easily be found. Now, where the difference is obvious, no difficulty, theoretically, need arise; but difficulty might occur if there were a difference that is not obvious. And nothing is easier than to pass from a subjective-direction to an objective-direction leading question without a suspicion that in doing so one is possibly employing the less reliable form of question. Of course, it is not obvious *a priori* which (if either) of the two question forms is, *e.g.*, the more reliable; but it is this very fact, combined with the practice of asking leading questions in either form, which makes it important to compare the influence of the two forms upon answers.

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[1] A reference to Lipmann's classification shews that his examples of the different types consist of objective-direction questions solely.

[2] Questions asked concerning introspections are always subjective-direction questions.

There is another element in the form of questions which has not hitherto been investigated. This element is the article. Does it make any difference to the answer to the question "Did you see *a* Zeppelin?" if the question be altered to "Did you see *the* Zeppelin?" This difference is not so trivial as it may perhaps seem. The question with the definite article certainly seems to be more 'implicative' than the question with the indefinite article, and consequently the small amount of verbal change in passing from one to the other may be connected with considerable changes in the character of the answers. Further, the two questions may be considered as loosely typical of questions with vaguely-defined and explicitly-defined content respectively, since it is generally assumed that the latter form of question is relatively 'implicative.'

In the present experiment, then, an attempt has been made to investigate (1) the influence of the direction (subjective or objective) of a question upon answers, (2) the influence of the articles upon answers. In addition, several question forms were arranged so that the influence of the negative upon answers might be made clear; and finally two question forms not connected with the above points were included in the experiment. These latter were the 'incomplete disjunction' and the 'implicative' question forms.

3. THE QUESTION FORMS INVESTIGATED IN THIS EXPERIMENT.

The following eight types of question have been investigated in the present experiment Under certain conditions they would all be considered leading questions. It will be noticed that the first four of them (*A*, *B*, *C*, *D*) are subjective-direction, and the last four (*W*, *X*, *Y*, *Z*) objective-direction questions. At the same time, only two subjective-direction questions (A, C) have precise correspondents among the objective-direction questions (W, X).

Direction of question	Reference letter of types	The question forms
Subjective	A	Did you see a...?
Subjective	B	Did you see the...?
Subjective	C	Did*n't* you see a...?
Subjective	D	Did*n't* you see the...?
Objective	W	Was there a...?
Objective	X	Was*n't* there a...?
Objective	Y	Was the K^1, m or n?
Objective	Z	Was the K^1, m?

If these types of questions be compared with the types given in Lipmann's classification (p. 353) it will be seen that the types marked A, B, and W, would be classified in Lipmann's scheme as *Yes-No* questions; that is, assuming that subjective-direction questions are to be included in Lipmann's classification. It will be seen also that the types C, D, and X, would be classified in Lipmann's scheme as *Yes* questions. Y is Lipmann's *incomplete disjunction* question and Z his *implicative* question. It is obviously difficult, however, to say what question is 'implicative' and what not. If Binet was right in supposing all questioning to be a forcing of memory, it would seem that all questions are more or less implicative. It is at least reasonable to consider the types marked B and D as implicative, because they contain the definite article; but these would not be considered as such in Lipmann's classification.

4. DESCRIPTION OF THE EXPERIMENT.

(*a*) *Observation material and apparatus.* Moving pictures[2] were used as material for observation. A number of photographs, which had been taken in rapid succession, were affixed, in the order in which they had been taken, to the outside of a small cylinder, as spokes to the hob of a wheel. By rotating the cylinder the separate photographs could be exposed in rapid succession. A cinematographic effect was thus produced. About 450 separate photographs went to the making

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[1] For information about 'K' see pp. 360 and 361–5.

[2] My thanks are due to Dr C. S. Myers for recommending the use of this kind of observation material, and for assisting in the selection of the films (see p. 357). The chief interest of such material, however, is connected with the free 'report,' which could not be dealt with in this paper.

of each moving picture. These pictures will be referred to as *films*. They were rotated by means of the motor of a memory apparatus which had been adjusted so that they could be fitted to it. The photographs measured approximately one inch by three-quarters of an inch; but they were observed through a magnifying lens which about doubled their size. They were observed from a distance of about six inches.

Five films[1] were employed in the experiment. These will be designated F_0, F_1, F_2, F_3, F_4.

F_0 need not be described at all, as it was used as a preliminary film only. It represented a practical joke.

F_1 gave a representation of conjuring tricks performed by one of two men. The men appeared first of all to be in a restaurant, and one of them seemed to be giving orders for refreshment to the other. In the course of the film a hat and a cigar were made to disappear, and a tea-service suddenly appeared from nowhere and as suddenly vanished.

F_2 shewed Marble Arch, London, in the background. In front of it was a part of Oxford Street along which various kinds of vehicles and a number of pedestrians passed in both directions.

F_3 shewed what appeared to be the main street of a fair-sized country town in England, and had as its central incident the yoking of horses to a coach which stood outside an inn.

F_4 depicted a practical joke played by a girl and two boys on a stout elderly man. The man was asleep in a basket chair on a lawn, and the girl was hosing the garden. The boys came up and induced the girl to turn the hose upon the sleeping man, after which they poured a bath of water over him[2].

(*b*) *Subjects*. Fifty-six subjects took part in the experiment, twenty-one women and thirty-five men. With the exception of three men and three women they were all university graduates or undergraduates. From the point of view of social standing and intellectual development the subjects thus constituted a fairly homogeneous group, or, if a distinction be made on the basis of sex, two homogeneous groups.

(*c*) *Procedure*. The time adopted for reeling off each film once was from 25″ to 28″; that is, each photograph was exposed about $\frac{1}{18}''$. The reason for the adoption of this constant rate was that with it the incidents appeared more life-like than with any other. If it was increased, objects began to blur; while if it was decreased, the film tended to shew gaps and jerks.

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[1] These films were manufactured by the Kinora Company, Letchworth, England.

[2] A more detailed description of the films than is given here does not appear necessary so far as this paper is concerned. A much fuller description would be required if the paper dealt with 'reports.'

At the beginning of the experiment the subject was shewn the preliminary film (F_0) and was asked to give an account of it. It was explained that this was not a part of the experiment proper, but was intended merely to indicate its general character. The preliminary film was shewn several times to each subject so that the conditions of the experiment might become familiar and adjustment might take place.

The subjects could not be informed concerning the chief aim of the experiment, as is obvious, *e.g.*, from the relation between subjective-direction and objective-direction questions. In no instance was the aim of the experiment suspected. After the preliminary film had been shewn, the subject was instructed as follows. "I will now shew you another, and I want you, after you have seen it, to tell me *everything* that you can remember of it." Another film was then shewn. After its exposure the subject dictated his report which was taken down *verbatim*. He was allowed to continue until he ceased spontaneously. No question was asked here, whatever gaps or additions the reports might contain.

The film was as a rule then shewn again, after which the subject reported additions to, or corrections of, his first report. Sometimes it was shewn a third time, and in rare instances a fourth.

Variations in the number of exposures depended upon the character of the films and differences between subjects. The principle determining the number of exposures in the case of any subject was that a certain grasp of the film was necessary before the previously constructed *questionnaire* concerning it could be put. It thus resulted that some subjects required more exposures of a film than others, and that most subjects required more exposures of certain films than of other films. It had to be determined roughly by the character of any report whether or not the subject had a sufficient grasp of the film to allow for detailed questions concerning it without further exposures.

The degree of knowledge of the film required from a subject before he was questioned was determined by the character of the questions and the purpose of the experiment. The aim of the experiment necessitated the elaboration, prior to the experiment, of a number of questions about the scenes depicted in the films, these questions being arranged in the different forms which were to be investigated. If a certain knowledge of the films was lacking, at least some of the prearranged questions could not be asked. Thus, one film (F_4) contained a bath as a fairly central object, and one of the prearranged questions concerned the position of this object at a certain time. This film was relatively easy, and consequently only one exposure of it was generally given before the questions were asked. One subject, however, did not mention a bath in his first report of this film, and it seemed evident that he had not seen it. Hence, to make the bath question possible for this subject, the film had to be shewn him a second time. And similarly in other cases.

When it appeared that a subject had acquired a requisite grasp of a film, the series of prearranged questions with regard to it was asked. The subject's answers were taken down *verbatim*, together with any spontaneous remarks he might make, and the attempt was generally made to obtain introspections.

All the questions were asked in as even a tone as possible, and the attempt was made to reduce suggestiveness from the tone of voice to a minimum.

The time taken for going through each film (report and *questionnaire*) was generally about one hour (each subject was taken separately). As a rule one film was taken in one sitting, but in certain cases two were taken. The twenty-one women each saw only two films. The men saw various numbers. Twenty-five saw four each. Three saw only one each, and the remainder saw three each. These numbers (of the films) are exclusive of the preliminary film.

(*d*) *Character of the questionnaire.* The *questionnaire* did not aim at 'completeness'; that is, no attempt was made to ask a question with regard to every object or incident or aspect of the films. The *desideratum* was to ask a certain number of questions in the different forms to be investigated.

In the construction of the *questionnaire* the principle was adopted that no point was to be enquired about if there was a probability that there would be a clear memory with regard to it, the presumption being that whenever the fact enquired about is, in the strict sense of the word, remembered, the form of the question has little if any influence in determining the answer. Consequently questions could not be asked when the information they might elicit was likely to be included in the report. Questions concerning prominent or fairly prominent objects or incidents were therefore, generally speaking, not included in the *questionnaire*.

The number of questions elaborated with regard to the various films is as follows: F_1, 16; F_2, 32; F_3, 32; F_4, 16. (To these, several additions were made later.) The eight question forms were distributed among the films as follows: F_1, two questions of each form; F_2, four of each form; F_3, four of each form; F_4, two of each form. This scheme allowed for twelve questions of each type. It was assumed that a comparison of the results obtained by varying the content in each question form twelve times would give some indication of the influence of the question form alone upon answers.

Half of the questions of the forms *A, B, C, D, W*, and *X*, were concerned with objects, etc., occurring in the films, the other half with objects, etc., *not* occurring in the films[1]. The '*K*' (cf. § 3) in the *Y* and *Z* forms was in every case some actually occurring object or event, and no question of these types was put to a subject unless the '*K*' in it had been observed by the subject. (This could generally be known from the subject's report.) Half the *Y* questions offered alternatives of which only one was true, the other half alternatives neither of which was true. The *m* (cf. § 3) in the *Z* questions was in half the questions an actual occurrence, in the other half a fiction[1].

It seemed necessary to vary the content of the questions in this way Otherwise the effects of mere guessing in one direction (*e.g.*, always "Yes") could hardly have been detected. And the conclusions concerning reliability might have been very misleading. The results fully justified the use of both true and false content, the false-content questions acting as 'controls' (cf. pp. 368, 372–3; and §§ 10–12, especially Table VI).

Not all the questions, as originally arranged, were put to every subject. Some subjects anticipated certain of these questions in their reports, and consequently such questions had to be excluded from the *questionnaire* so far as these subjects were concerned. This fact helps to explain why in any one film all questions are not asked the same number of times (see tables of questions below; *e.g.* F_1 1 is asked 55 times, F_1 2, 47 times).

5. THE INDIVIDUAL QUESTIONS AND THEIR ANSWERS.

The *questionnaire* will now be given together with the analysed answers for each question. The answers are placed in three columns, the right (R), the wrong (W), and the "I do not know" (?). The classing of an answer was sometimes a matter of considerable difficulty. The classification employed depends upon the attempt made in the experiment to compare the objective validity of the information given in answer to questions of the subjective-direction form, with that of the information given in response to questions of the objective-direction form. Consequently, an answer is considered right if it asserted, *or implied*, the objective occurrence of the object, etc., enquired about, if such object, etc., did occur, or the *non*-occurrence of such object, etc., if it did *not* occur. An answer is considered wrong in parallel but reverse conditions. *All other answers* are considered, from the point of view of the objective occurrence of the object, etc., enquired about, equivalent to "I do not know." Thus, an answer "Yes" to a 'Did you see?' question is *either* right or wrong, since it *implies* that the object, etc., enquired about occurred; but the answer "No" to such a question is *neither* right nor wrong, as it implies nothing (and of course *asserts* nothing) concerning the occurrence of the object, etc., enquired about. Such an answer, then (that is, "No" to a 'Did you see?' question), has been placed in the '?' class.

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[1] These questions will be designated respectively 'questions with true content' and 'questions with false content.'

The Questionnaire.

The questions were generally asked in the order in which they are numbered.

Note. In the questions with regard to F_1, A indicates the conjurer, B the man who was seated.

p before the letter indicating a question type signifies that the object or event asked about was *present* in the film, *n* that it was *not* present.

Ref. No. of Q.	Type of Q.	Questions asked concerning F_1	Total No. of times asked	Analysis of answers		
				R	W	?
*1_3	*nW*	Was there a third man anywhere?	55	37	0	18
2	*pA*	Did you see an umbrella?	47	7	2	38
3	*pX*	Wasn't there a dark pattern on the table cloth?	45	9	13	23
4	*nZ*	Was the hair of *B* very dark?	53	29	13	11
5*a*	*pY*	Was *A*'s tie black or white?	54	17	13	24
5*b*	*nB*	Did you see *A* light the cigarette?	43	6	6	31
6	*nB*	Did you see *A* return the cigarette to *B*? ...	49	6	7	36
*7_3	*pC*	When the hat disappeared, didn't you see a puzzled expression on *B*'s face?	53	36	1	16
8	*nY*	When the hat disappeared, did *B* shrug his shoulders or did he rise from his chair? ...	51	16	13	22

* For the meaning of the asterisks and suffixes to the reference numbers, see below (p. 368).

Ref. No. of Q.	Type of Q.	Questions asked concerning F_1 (continued)	Total No. of times asked	Analysis of answers R	W	?
9	*pD*	After the hat had gone, didn't you see the gesture by which B indicated to A that he wanted something to drink?	47	4	1	42
10	*nC*	Didn't you see a cup moved quickly by A from underneath the napkin?	49	0	7	42
11	*pW*	Was there a milk-jug produced by A? ...	51	10	18	23
*12_2	*nX*	Wasn't there a piece of cake produced by A?	54	31	0	23
13	*pZ*	Did B put sugar into his tea?	49	6	23	20
*14_3	*nA*	Did you see a very surprised expression on B's face when the tea things disappeared?	51	2	35	14
*15_3	*pB*	Did you see the way in which A put his hand to his mouth several times, as though to hide a smile from B?	53	30	3	20
*16_2	*nD*	Didn't you see the picture on the wall? ...	54	10	1	43
17	*nW*	Was there a flower in B's coat?	49	13	3	33

Ref. No. of Q.	Type of Q.	Questions asked concerning F_2	Total No. of times asked	Analysis of answers R	W	?
1	*pA*	Did you see a taxi passing on the far side of the arch?	47	12	2	33
2	*pW*	Was there a bath chair among the traffic at any point?	48	5	11	32
*3_2	*nC*	Didn't you see a hansom?	51	3	1	47
*4_2	*pB*	Did you see the dog?	47	2	2	43
5	*pX*	Wasn't there a lamp-post and street island on the right?	41	12	5	24
*$5a_1$†	*pY*	Was all of the lamp-post visible, or only a part of it?	18	11	3	4
6	*nD*	Didn't you see the little girl crossing, from right to left, towards the arch?	50	0	5	45
*7_3	*nW*	Was there a mounted soldier?	53	30	1	22
*8_3	*nA*	Did you see a man on a bicycle?	48	1	18	29
*9_2	*pD*	Didn't you see the lady's bonnet passing in the foreground from left to right? ...	28	16	0	12
10	*nY*	Was the bonnet trimmed with feathers or with ribbon?	38	6	18	14
11	*nX*	Wasn't there a white horse drawing a waggon?	51	6	25	20
*12_2	*pC*	Didn't you see a van with a grey canvas covering?	24	13	0	11
13	*nB*	Did you see the taxi standing in front of the arch?	50	4	3	43
14	*pY*	Were there two prams, or were there three?	37	6	13	18
*15_2	*nY*	Was the last 'bus numbered seventeen or twenty-seven?	49	0	1	48
*16_3	*pZ*	Was a nurse who pushed one of the prams dressed in white?	41	28	2	11
17	*pW*	Was there a lull in the heavy traffic towards the end of the film?	36	26	5	5

† This is a *complete* disjunction question. It was, however, eliminated for other reasons.

Ref. No. of Q	Type of Q.	Questions asked concerning F_2 (continued)	Total No. of times asked	Analysis of answers R	W	?
18	*pA*	Did you see a man who was driving a waggon smoking a pipe?	52	6	0	46
*19_2	*nC*	Didn't you see a lady on a bicycle?	48	2	0	46
*20_1	*pB*	Did you see the man on the provision tricycle?	5	3	0	2
21	*nZ*	Was he dressed in black?	42	16	18	8
22	*pX*	Weren't there a number of people walking immediately in front of the arch, and not crossing the street?	49	16	8	25
23	*nD*	Didn't you see the policeman?	47	1	4	42
24	*nW*	Was there a big waggon-load of coal? ...	51	21	11	19
*25_2	*nA*	Did you see an open parasol, held by a lady in a carriage?	51	1	18	32
26	*pY*	Was there one carriage, or were there two? ...	32	10	11	11
27	*pD*	Didn't you see the man in the foreground, walking by the side of the vehicles? ...	34	9	0	25
28	*nX*	Wasn't there a motor cycle at one point in the traffic?	50	17	2	31
29	*pC*	Didn't you see a tree to the left of the arch?	40	15	2	23
30	*nB*	Did you see the workmen digging up the street?	51	6	0	45
31	*pZ*	Were there just two 'busses?	49	12	23	14
32	*nZ*	Was the last 'bus without passengers on top?	50	11	12	27

Ref. No. of Q.	Type of Q.	Questions asked concerning F_3	Total No. of times asked	Analysis of answers R	W	?
1	*nW*	Was there a policeman standing on the footpath near the coach?	27	10	2	15
2	*pA*	Did you see a diamond-shaped sign fixed to the balcony of the inn?	25	4	0	21
3	*pX*	Wasn't there a little girl dressed in white walking along the footpath in front of the inn?	17	3	1	13
4	*nZ*	Was the boy on the bicycle wearing a hat? ...	22	12	8	2
*5_1	*pB*	Did you see the man pass across in the foreground out of focus?	12	4	1	7
*6_1	*pY*	Were there two men out of focus or were there three?	7	2	2	3
7	*nB*	Did you see the lamp-post at the rear of the coach?	26	0	6	20
8	*pC*	Didn't you see a street-lamp fastened to the balcony of the inn?	19	2	0	17
*9_1	*pY*	Were there two street-lamps, or were there three?	5	2	3	0
*10_1	*pD*	Didn't you see the plants on the balcony of the inn?	13	5	0	8
11	*pW*	Was there a white horse among the team in the coach?	17	9	6	2
12	*nC*	Didn't you see a flag hanging out from the balcony of the inn?	25	1	2	22
*13_1	*nY*	Were there two women on top of the coach, or were there three?	12	1	2	9

Ref. No. of Q.	Type of Q.	Questions asked concerning F_3 (continued)	Total No. of times asked	Analysis of answers		
				R	W	?
*$14a_1$	*nX*	Wasn't there a woman standing on the balcony of the inn, watching the coach?	11	3	0	8
14*b*	*nX*	Wasn't there a horse and dray in the background?	22	6	2	14
15	*pZ*	Was the man on the bicycle wearing cycling stockings?	34	6	11	17
16	*pZ*	Was he wearing a hat?	14	7	3	4
17	*nA*	Did you see a nurse with a pram?	27	0	4	23
18	*nD*	Didn't you see the man who lifted a portmanteau into the coach?	27	1	2	24
*19_3	*nW*	Was there a church-tower visible on the extreme right of the picture?	27	11	0	16
20	*pB*	Did you see the coachdriver's whip?	26	5	0	21
21	*pX*	Wasn't there a part of the side of the coach painted white?	24	10	8	6
22	*nZ*	Was the pavement dark in colour as compared with the street?	27	4	10	13
23	*pY*	Were there two men in long coats and tall hats or were there three?	25	12	11	2
24	*nB*	Did you see the dark stripes on some of the shop-awnings?	16	4	3	9
*25_1	*pW*	Was there a shop-awning that reached almost to the top of the picture?	13	4	4	5
26	*pC*	Didn't you see a dealer's hand-cart in front of the shops at the rear of the coach?	22	2	1	19
*27_1	*pD*	Didn't you see the two men with shirt sleeves rolled up who yoked the horses to the coach?	8	0	2	6
28	*nX*	Wasn't there a horse standing in front of the inn after the coach drove off?	26	9	2	15
*29_2	*nD*	Didn't you see the woman dressed in white?	23	0	1	22
*30_2	*nA*	Did you see a man rush out of the inn and then rush back again?	26	0	2	24
31	*nC*	Didn't you see a man wearing a tradesman's apron?	25	0	7	18
*32_3	*pB*	Did you see the bystanders watching the coach as it disappeared?	27	12	0	15

Ref. No. of Q.	Type of Q.	Questions asked concerning F_4	Total No. of times asked	Analysis of answers		
				R	W	?
1	*nW*	Was there a second girl watching the affair from behind some shrubbery?	25	6	0	19
2*a*	*pB*	Did you see the man's head?	21	6	0	15
2*b*	*pB*	Did you see the newspaper lying on the man's lap?	23	6	1	16
3	*pX*	Wasn't there a stripe in the girl's dress?	25	8	5	12
4	*nZ*	Was there a dark hem on her skirt?	25	9	1	15
5	*pY*	Were both boys wearing caps, or only one?	21	14	2	5
6	*nB*	Did you see the man's coat lying by the side of the chair?	25	1	3	21

Ref No. of Q.	Type of Q.	Questions asked concerning F_4 (continued)	Total No. of times asked	Analysis of answers		
				R	W	?
7	*pC*	Didn't you see a small part of a house in the background?	21	5	0	16
8	*nY*	Was the bath left lying on its side, or turned upside-down?	25	4	6	15
9	*nD*	Didn't you see the white flowers on the shrubs in the right of the picture?	24	1	2	21
10	*pW*	Was there a school badge on one of the boy's caps?	21	6	3	12
11	*nC*	Didn't you see a cushion fall out of the chair as it turned over?	25	0	4	21
12	*nX*	Wasn't there a piece of light ribbon in the girl's hair?	25	5	2	18
$*13_2$	*pZ*	Did the boys empty the bath over the man after the girl had sprayed him?	24	24	0	0
$*14_2$	*nB*	Did you see the man push the tub away with one hand, as he got up?	24	0	1	23
15	*pB*	Did you see the water shooting out of the hose, while the girl was talking to the boys?	25	12	6	7
$*16_2$	*nD*	Didn't you see the boy on the left pull off the man's hat before the girl sprayed him?	23	0	1	22

6. EXCLUSION OF CERTAIN OF THE ORIGINAL QUESTIONS.

If the results (analysed answers) for the individual questions were now to be combined into groups each of which consisted of the answers to all the questions of some one type, we should obtain eight groups of answers, corresponding to the eight question types; and each group would appear as analysed into right, wrong, and "I do not know" answers. It might be thought that a general comparison of the question forms in respect of their influence upon answers could then be made from these crude aggregates. Such a comparison, however, would be possible only if the influence of any non-mental determinants of answers (other than question form) had been the same for each group of questions. There is evidence that with regard to one such determinant, namely, question content, this was not the case. Notwithstanding that in the construction of the *questionnaire* an attempt was made to render the influence of question content constant, it would appear that almost the sole non-mental determinant of the answers to certain questions was the content of the questions. The answers to Q. F_4 13, *e.g.*, shew 100 % right. In contrast, the answers to Q. F_1 14 shew 69 % wrong, while over 95 % "I do not know" answers were returned to Q. F_4 14. Such results are not due to question form since other questions in the same forms as these three, which are of different forms, do not exhibit a similar special tendency in one direction. Further, remarks made spontaneously by subjects seem to indicate that whenever there occurred a tendency for the answers to any question to fall into some one of the three classes (right, wrong, and "I do not know"), the influence of question content in determining the answers was paramount. It will be seen by glancing over the above results for the answers that such tendencies occurred fairly often.

Consequently, if the results for the individual questions were to be at once combined into groups according to question type, we should not obtain any true indication of the effects of different question forms *unless the influence of question content was the same for each group.* An examination shews that this condition is not satisfied, some groups containing more questions of which the answers seem to be determined by content chiefly than do others. An attempt must therefore be made to equalize the effect of question content in the different groups before the question forms are compared. For this purpose the following procedure was adopted, and although it is somewhat arbitrary, it has the merit of eliminating the most pronounced instances of determination of answers by content of questions.

The procedure consisted of applying the three following rules.

(1) *Any question is to be eliminated from the experiment if asked less than* 50 % *of the possible number of times* (that is, if put to less than half the subjects who were questioned about the film with which it was connected).

This rule served to remove certain questions that were either too easy or too hard (because of content). If a question was not asked more than half the possible number of times, this was because, under the conditions of the experiment, *either* the object, etc., enquired about (in the question) had been frequently mentioned in the reports, *or* the question presupposed a certain knowledge of the film, which, in fact, was frequently lacking. The presumption is that in the former case the question was too easy because of the content, in the latter case too hard. For example, Q. F_2 20 was asked five times out of a possible fifty-three. The content of this question indicated a relatively conspicuous object which was mentioned in most of the reports. The probability thus is that it was also observed by the few subjects who did not mention it. Again, Q. F_3 6 was asked seven times out of a possible twenty-seven. It enquired about the "number of men out of focus," whereas many subjects observed no such men. The content here, then, rendered the question 'too hard.'

(2) *Any question is to be eliminated to which "I do not know" answers were returned* 90 % (*or more*) *of the number of times it was asked.*

This rule served to remove certain questions that were too hard. For if 90 % (or more) of the persons who are asked a question reply to it "I do not know," it may be assumed that they consciously recognize the object, etc., enquired about to be outside the range of their knowledge; and in such instances the answers appear to be determined by content chiefly.

(3) *Any question is to be eliminated with regard to which*

$$\frac{\text{Right Answers}}{\text{Wrong Answers}} = 10 \ (\textit{or more}), \text{ or } \tfrac{1}{10} \ (\textit{or less}),$$

provided that either (*a*) *at least ten Right answers are returned to the question, or* (*b*) *at least ten Wrong answers are returned to it.*

The reason for this rule is that when $\frac{R}{W} = 10$ (or more) the question is too easy, and the answer is apparently determined by question content chiefly (as, *e.g.*, when a question concerns an occurrence which a subject considers too obvious to report); and when $\frac{R}{W} = \frac{1}{10}$ (or less) the question may be considered too hard, and is apparently determined by 'suggestibility' of question content. The reason for the provision that at least ten right or wrong answers must be returned before the rule is applied is that it is necessary to be *sure* of the drift of the tendency before elimination, it being assumed that this number (ten), together with the ratio employed, gives good ground for supposing a special tendency.

By the application of these three rules, we have excluded from the results upon which a comparison of the influence of different question forms will be based about one-third of the questions used in the experiment[1]. So far as the chief object of the experiment is concerned, this is rather unfortunate, especially as the questions which remain after elimination are not equally divided among the eight forms. Greater care in the construction of the questions, with a view to making the influence of question content constant, would probably have reduced considerably the number of questions which had to be eliminated. At the same time, it could hardly be determined *a priori* whether or not some questions might be unduly weighted by content; and consequently in similar circumstances some elimination of questions *after the experiment* will generally be necessary. Indeed, in a sense it is only the results that can shew which questions are unduly weighted by content.

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[1] In the course of the experiment, approximately 3400 questions were asked.

In the tables of the questions (above), the reference numbers of eliminated questions are marked by asterisks; and the number of the rule (1, 2, or 3) by the application of which any question is eliminated is suffixed to the reference number of that question.

7. ANALYSIS OF ANSWERS FOR EACH QUESTION FORM.

Having eliminated the worst instances of determination of answers by content of questions, let us proceed with a consideration of the questions that remain. The answers of all the questions of each form will be aggregated to form one group, the right answers returned to any one question of one form being added to the right answers returned to all the other questions of that form, and similarly for wrong and "I do not know" answers. The results are set out in the three following tables. In Table I are shewn the results for the true-content[1] questions of each form; that is, for those questions marked in the question tables with a *p*. In Table II are shewn the results for the false-content[1] questions of each form; that is, for those questions in the question tables marked with an *n*. In Table III are shewn the results for *all* questions of each form (both true-content and false-content questions).

The percentages are calculated to the nearest whole number.

TABLE I. *Combined results for true-content questions of each form.*

Type of Q.	Questions used for results (after elimination for content)			Analysis of answers in %		
	No.	Reference Nos.	No. of times asked	R	W	?
(*p*) *A*	4	F_1 2; F_2 1, 18; F_3 2	171*	17	2	81
(*p*) *B*	4	F_3 20; F_4 2*a*, 2*b*, 15	95	31	7	62
(*p*) *C*	4	F_2 29; F_3 8, 26; F_4 7	102	23	3	74
(*p*) *D*	2	F_1 9; F_2 27	81	16	1	83
(*p*) *W*	5	F_1 11; F_2 2, 17; F_3 11; F_4 10 ...	173	32	25	43
(*p*) *X*	6	F_1 3; F_2 5, 22; F_3 3, 21; F_4 3 ...	167	38	28	34
(*p*) *Y*	4	F_1 5*a*; F_2 14; F_3 23; F_4 5	137	36	28	36
(*p*) *Z*	4	F_1 13; F_2 13; F_3 15, 16	136	23	44	33

* 171 = the no. of times F_1 2 was asked (47), + the no. of times F_2 1 was asked (47), + the no. of times F_2 18 was asked (52), + the no. of times F_3 2 was asked (25); and similarly for the other numbers in the same column as 171.

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[1] See note to p. 360.

TABLE II. *Combined results for false-content questions of each form.*

	Questions used for results (after elimination for content)			Analysis of answers in %		
Type of Q.	No.	Reference Nos.	No. of times asked	R	W	?
(*n*) *A*	1	F_3 17	27	0	15	85
(*n*) *B*	7	F_1 5*b*, 6; F_2 13, 30; F_3 7, 24; F_4 6	260	10	11	79
(*n*) *C*	4	F_1 10; F_3 12, 31; F_4 11	124	1	16	83
(*n*) *D*	3	F_2 23; F_3 18; F_4 9	98	3	8	89
(*n*) *W*	4	F_1 17; F_2 24; F_3 1; F_4 1	152	33	11	56
(*n*) *X*	5	F_2 11, 28; F_3 14*b*, 28; F_4 12 ...	174	25	19	56
(*n*) *Y*	3	F_1 8; F_2 10; F_4 8	114	23	32	45
(*n*) *Z*	6	F_1 4; F_2 21, 32; F_3 4, 22; F_4 4 ...	219	37	28	35

TABLE III. *Combined results for all questions of each form.*

	Questions used for results (after elimination for content)			Analysis of answers in %		
Type of Q.	No.	Reference Nos.	No. of times asked	R	W	?
A	5	Nos. in Table I + Nos. in Table II	198	15	4	81
B	11	,, ,,	355	16	10	74
C	8	,, ,,	226	11	10	79
D	5	,, ,,	179	9	5	86
W	9	,, ,,	325	32	18	50
X	11	,, ,,	341	31	24	45
Y	7	,, ,,	251	30	30	40
Z	10	,, ,,	355	32	34	34

The percentages in this table have been calculated from the absolute figures; that is, they have not been arrived at by combining the percentages in Tables I and II.

It will be seen that the rules for elimination (pp. 366–8) have left in Table I only two *D* questions and in Table II only one *A* question. Too much importance should not therefore be attached to small differences or similarities in connexion with these two types, since, the questions being few, the answers may indicate the influence of question content rather than of question form. For although the remaining questions were not excluded by the elimination rules, it must be remembered that these rules were intended to exclude only the most pronounced instances of determination of answers by question content. Consequently any remaining question *may* have its answers chiefly determined by its content, even though it escaped the exclusion rules. The more questions of each form compared, the more will differences due to content be eliminated (as a general rule).

8. PLAN OF DISCUSSION OF THE RESULTS.

There are three important qualities with regard to which question forms may be compared. These are (*a*) caution, (*b*) suggestiveness, and (*c*) reliability.

(*a*) *Caution* is measured in this paper by the percentage ratio of the "I do not know" answers to the total number of answers. Thus the caution connected with the *A* and *Z* question forms in Table III is represented by the figures 81 and 34 respectively. It has not been usual to compare question forms with regard to caution.

(*b*) *Suggestiveness.* Whether or not there is any single mental process that can significantly be called suggestion, no such process is indicated in the present connexion. By the suggestiveness of a question form is understood here its relative capacity to produce as answers assertions that the object, etc., enquired about was *present* in the observation material (or equivalent assertions. In the case of the *Y* question form, these equivalents are either of the alternatives mentioned in the question). The quality thus indicated is of interest and importance. We shall take as a comparative measure of it the percentage ratio which the answers *asserting* the occurrence of the object, etc., enquired about bears to the total made up of *these*[1] answers and *those*[1] *denying* the occurrence of such object, etc. Omitting the *Y* form, assertions of presence make up all the right answers in Table I and all the wrong answers in Table II, while the reverse is true of denials of presence.

With regard to the *Y* form, assertions of presence make up all the right answers in Table I and also *part* of the wrong answers,—that part, namely, which consists of answers accepting the wrong alternative. Some wrong answers here, namely those not accepting the wrong alternative, are denials of presence, since they reject both of the alternatives offered. With regard to Table II, denials of presence make up all the right answers, since both alternatives offered here were fictitious; assertions of presence (that is, assertions that one of the alternatives offered is correct) all the wrong answers. Special analysis of the answers to the *Y* questions has been made, in view of this complication.

Suggestiveness may then be defined as $\frac{P}{P+N}$ (in %), where P is the number of *assertions* of presence, and N the number of *denials* of presence (the necessary modifications for the *Y* form being understood). In considering suggestiveness as thus defined the question of the rightness or wrongness of any assertion or denial of presence is not taken into account. *P*, for example, will be made up of both right and wrong answers.

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[1] Such answers will be referred to as 'assertions of presence' and 'denials of presence' respectively.

(*c*) *Reliability* is a now recognized measure. It is defined as $\frac{R}{R+W}$ (in %), where *R* is the number of right answers and *W* the number of wrong answers. (It should be remembered that in the present experiment rightness or wrongness of answers is estimated from the point of view of the objective occurrence of the object, etc., enquired about. See pp. 361, 370.)

I shall first compare subjective-direction with objective-direction question forms in a general way with respect to caution, suggestiveness, and reliability. I shall then compare the specific question forms, endeavouring to discover the influence upon caution, suggestiveness, and reliability of (1) converting the indefinite into the definite article (or *vice versa*) in a question form, (2) introducing a negative into a question form, and (3) altering the general direction of a question form (from subjective-direction to objective-direction, or *vice versa*) while keeping it otherwise the same.

9. SUBJECTIVE-DIRECTION COMPARED GENERALLY WITH OBJECTIVE-DIRECTION QUESTION FORMS IN RESPECT OF (A) CAUTION, (B) SUGGESTIVENESS, (C) RELIABILITY.

A. *Caution.* It is noticeable that the caution connected with subjective-direction question forms is (in the three preceding tables) considerably greater than that connected with objective-direction question forms. In Table I the highest and lowest percentages of "I do not know" answers for objective-direction question forms are 43 and 33 respectively, while analogous figures for subjective-direction forms are nearly double these, namely, 83 and 62. A similar relation obtains in Table II (and consequently in Table III). It may therefore be said that caution occurred in connexion with subjective-direction question forms *much* more than in connexion with objective-direction question forms. Can any explanation of this fact be offered?

It might be first thought that to ask any question in the form "Did you *perceive* (such-and-such a thing)" instead of in the form "Was there (such-and-such a thing)" is to choose the more *searching* of the two question forms. It might be thought that the latter form would lead the person who is questioned to calculate, on the basis of his observation and past experience, the *probability* of the occurrence of the object, etc., enquired about, thus causing a possibly rash answer. In contrast, the subjective-direction question form insists that the experience of cognizing the object, etc., enquired about shall be the *only* criterion for deciding as to its occurrence, thus giving rise to considerable caution. According to this idea the conversion of an objective-direction into a subjective-direction question might be considered as adding to the former question an injunction equivalent to "Now, be very careful!"

There is certainly some truth in this explanation of the figures, as will be seen presently (pp. 373–4). The fact is however that subjective-direction questions, in comparison with objective-direction questions, *allow for* as well as *produce* caution.

Consider the question "Did you see a dog?" as typical of subjective-direction question forms, and the question "Was there a dog?" as typical of objective-direction forms. To the question "Did you see a dog?" the answer given in any case is, generally speaking and in essence, one of the following: (1) Yes; (2) No; (3) I do not know; (4) There was no dog to be seen. The first two of these occurred *far more* frequently in the course of the experiment than the last two. Again, to the question "Was there a dog?" the answer given in any case is, generally speaking and in essence, one of the following: (1) Yes; (2) No; (3) I did not see one; (4) I do not know. And of these answers also the first two occurred *far more* frequently than the last two. It is therefore reasonable to assume the existence of a strong tendency to reply to questions of both forms, "Did you see a dog?" and "Was there a dog?" by a simple "Yes" or "No." It is as if one had said, after asking a question in either of these forms, "Answer me in one word—Yes or No." Assuming the existence of such a tendency (which seems verified by the relative frequencies of the different answers), it is possible to explain the great difference between subjective-direction and objective-direction question forms in respect of caution (the proportions of "I do not know" answers).

The answer "Yes" to the question "Did you see a dog?" is either right or wrong, *judged from the point of view of the occurrence of a dog.* Analogously, the answer "Yes" to the question "*Was there* a dog?" is either right or wrong. But the case is different with regard to the answer "No." This answer, if returned to the question "*Did you see* a dog?" is, from the point of view of the occurrence of a dog, neither right nor wrong (although, as to the question of perception, it may be correct). It asserts merely that a dog was *not seen*, and its implication is "I do not know whether or not a dog was present." When the answer "No" is returned to a subjective-direction question it must therefore be placed in the "I do not know" class. On the other hand, when it is returned to the question "*Was there* a dog?" it is either right or wrong, just as is the answer "Yes." That is, the answer "Yes" to both questions, "*Did you see* a dog?" and "*Was there* a dog?" is either right or wrong; but the answer "No," while either right or wrong when returned to the latter question, is equivalent to "I do not know whether or not there was a dog" if returned to the former.

If it be assumed, as seems reasonable, that the Yes-No tendency is roughly of the same strength whether it occurs in connexion with subjective-direction or objective-direction question forms, the relation between the two forms in respect of caution can readily be understood; for more answers equivalent to "I do not know" will then be given to subjective-direction than to objective-direction questions. Indeed, the Yes-No tendency produces *no* "I do not know" answers in reply to objective-direction questions. That is, the giving of answers equivalent to "I do not know" is in accord with and furthered by the Yes-No tendency in the case of subjective-direction questions, whereas such answers must be made in opposition to this tendency in the case of objective-direction questions. In answer to an objective-direction question, a person is led, by the Yes-No tendency, to assert *more than* his experience will justify; to assert that the object, etc., did *not occur* when the fact is simply that it was *not perceived*. If the person desires to state the facts with precision, he must overcome the Yes-No tendency, and give an answer equivalent to "I do not know."

But although subjective-direction questions, as compared with objective-direction questions, in virtue of the Yes-No tendency, *allow for* caution, it would appear that a certain amount of caution is also *produced* by them. This is seen from the following considerations. The caution that is allowed for by the subjective-direction forms occurs in connexion with "No" answers only. Consequently this caution does not occur in connexion with the right answers in Table I and the wrong answers in Table II, since these are constituted of "Yes" answers. If, however, subjective-direction be compared with objective-direction forms in respect of these two columns of answers, *it will be found that the former exhibit more caution than the latter*. The mean for the subjective-direction question forms (Table I, column R) is 22, that for objective-direction question forms (omitting *Y*, because the Yes-No tendency does not occur in its purity in connexion with it) being 31. Similarly, comparing the same forms for the column of wrong answers in Table II, the figures are 12·5 and 19·3.

This means that the caution connected with subjective-direction question forms is considerably greater than that connected with objective-direction forms even in respect of those answers (the "Yes" answers) that are similarly affected in both forms by the Yes-No tendency. This greater caution, not being 'allowed for' by the subjective-direction forms, is therefore in some way or other *produced* by them. The above figures shew that the mean for the caution connected with objective-direction forms (*W*, *X*, and *Z*) is, in the columns where caution is not 'allowed for,' approximately only two-thirds of that connected with subjective-direction forms (*A*, *B*, *C*, and *D*). It may therefore be concluded that the subjective-direction is the more searching form of question.

To sum up: answers to subjective-direction questions exhibit *much* more caution than answers to objective-direction questions (so far as the particular forms investigated are concerned). This is partly due to the tendency to answer questions of either form (excluding *Y*) by a simple "Yes" or "No," this tendency being opposed to caution in the case of objective-direction questions while allowing for and furthering it in the case of subjective-direction questions. It is also partly due to the fact that a subjective-direction question is more *searching* than an objective-direction question, thus being *productive* of the more caution, as can be seen by consideration of the "Yes" answers[1].

Whether subjective-direction questions, in raising the threshold for the making of a definite declaration as to an object's occurrence, increase the reliability of the answers is a difficult question which will be considered below (pp. 381 *et seq.*).

B. *Suggestiveness.* Before making a general comparison of subjective-direction and objective-direction question forms with regard to suggestiveness (and also with regard to reliability), the results will be arranged in more convenient form in the following table (IV).

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[1] Comparing the right and wrong answers; it is noticeable that the former are much greater than the latter in Table I for the subjective-direction forms, and generally speaking much smaller in Table II, while no such relation holds between right and wrong answers for objective-direction forms in either table (with the possible exception of *W* in Table II). The explanation of this is to be found in the preceding considerations with regard to caution. The wrong answers to subjective-direction forms in Table I are relatively small because the "No" answers to these questions went into the "I do not know" column, and similarly for the right answers in Table II. This did not happen to the corresponding answers to the objective-direction forms, and the right and wrong answers to these forms are therefore more nearly equal.

TABLE IV*. *Table shewing Results for Caution, Suggestiveness, and Reliability, etc.*

Type of Q.	Total no. of assertions† and denials of presence	No. of assertions and denials of presence as percentage of total no. of assertions		Total no. of right and wrong answers	Caution	Suggestiveness	Reliability
		Assertions	Denials				
A	37	16·6	2	37	81	89·2	78·4
B	91	16	9·5	91	74	62·6	61·5
C	48	19·5	1·8	48	79	91·7	52
D	25	11·7	2·2	25	86	84	64
W	165	22·2	28·6	165	50	43·6	64·2
X	187	28·4	26·4	187	45	51·8	57
Y	151	46·6	13·5	151	40	77 5	49·7
Z	234	26·2	39·7	234	34	39·7	47·8

* The percentages here are calculated throughout from the absolute figures, not from other percentages (*e.g.* those in Tables I and II, which could be used for reliability).

The identity of the figures in the second and fifth columns from the left hand side of the table results from the fact that only assertions and denials of presence have been considered right or wrong.

† See note p. 370.

With one exception (*Y*), the suggestiveness of subjective-direction is much greater than that of objective-direction question forms. This does not mean that a much greater proportion of assertions of presence occurred among the answers to the former than among those to the latter type of questions. As will be seen from Table IV, the proportion of assertions of presence is actually greater for objective-direction than for subjective-direction question forms. But relatively few denials of presence were made in answer to subjective-direction question forms This is intelligible when it is borne in mind that a denial of presence, if made in answer to a subjective-direction question form, has to oppose the Yes-No tendency (p. 372).

Thus, although the proportions which the 'assertions *plus* denials' of presence form of the totals of answers are respectively less for subjective-direction than for objective-direction question forms, the proportion of denials of presence is relatively so *much* smaller than that of the assertions of presence in the former type that the suggestiveness of this type becomes relatively very high.

The suggestiveness of the *Y* type, although relatively high, is nevertheless lower than that of any subjective-direction question form with the exception of the lowest (*B*). The assertions of presence given in answer to *Y* questions are very numerous; that is, there is a strong tendency to accept *one or other* of the alternatives offered in the question. It is interesting to note that the suggestiveness of the *Y* question form is nevertheless lower than that of most of the subjective-direction question forms, the explanation being that the proportion of denials of presence for these forms is much smaller than that for *Y*.

C. *Reliability*. There is no very clear difference between the subjective-direction and objective-direction forms in general with regard to reliability. It will be seen, however, that a subjective-direction form (*A*) carries the highest reliability, while two objective-direction forms (*Y*, *Z*) carry the lowest. The mean reliability for the subjective-direction forms, again, is approximately 64, that for the objective-direction forms 54·7. At the same time, one objective-direction form (*W*) is higher in respect of reliability than any subjective-direction form with the exception of *A* (although it is *very* slightly higher than one other, namely, *D*). Further, the reliability of *X* is greater than that of *C*; but *C* is the lowest of the subjective-direction forms in respect of reliability. From these facts we may conclude that subjective-direction question forms are possibly slightly more reliable than objective-direction forms. Any more definite statement concerning reliability will be postponed, as the difficulties connected with this measure are considerable (see below pp. 379 *et seq.*, and the final section).

10. THE SPECIFIC QUESTION FORMS COMPARED.

We proceed, now, to a comparison of the specific question forms in respect of caution, suggestiveness, and reliability. For the purpose of this comparison the results have been arranged in the following table (V). The relative sizes of the differences indicated in this table by the 'greater than' and 'less than' signs can readily be calculated from Table IV.

The question forms to be specially compared are the following: *A* with *B*, and *C* with *D*, with respect to the influence of the article; *A* with *C*, *B* with *D*, and *W* with *X*, with respect to the influence of the negative; and *A* with *W*, and *C* with *X*, with respect to the specific influence of the direction of question form.

It will be noticed that in five instances (Table V) the sign '<' is thickened. This is to emphasize the fact that the influence of the specific factors of the question forms appears to be different in some cases from what it is in others. Thus, consider the case where the thickened sign is marked 1. C is similar to A in that both contain the indefinite article, and D is similar to B in that both contain the definite article. When however we compare A with B and C with D we find that, although in respect of caution A is *greater than B,* the caution connected with C is *less than* that connected with D. A similar relation holds between these pairs of question forms in respect of reliability. This fact is indicated by the thickening of the sign '<' in both instances. Again, consider the case where the thickened sign is marked 2. If the influence of the negative were the same in all instances we should expect that in respect of caution B would have been greater than D, for (in respect of caution) $A > C$ and $W > X$. But the reverse is the case; and as this is an apparent contradiction of the tendency exhibited in the other two pairs of question forms, it is emphasized specially (by the thickening of the sign '<'). Similar remarks apply to the comparisons of B with D and C with X in respect of reliability.

TABLE V.

	Type of Question		Specific characteristics (in italics)	Results		
Points for comparison	General	Specific		Caution	Suggestiveness	Reliability
Influence of difference in article	S–D*	A	...*a*...	$A>B$	$A>B$	$A>B$
	S–D	B	...*the*...			
	S–D	C	(not)...*a*...	$C \boldsymbol{<}^{1} D$	$C>D$	$C \boldsymbol{<} D$
	S–D	D	(not)...*the*...			
Influence of the negative	S–D	A	...(a)...	$A>C$	$A<C$	$A>C$
	S–D	C	...*not* .(a)...			
	S–D	B	...(the)...	$B \boldsymbol{<}^{2} D$	$B<D$	$B \boldsymbol{<} D$
	S–D	D	...*not*...(the)...			
	O–D	W	...(a)...	$W>X$	$W<X$	$W>X$
	O–D	X	...*not*...(a)...			
Influence of the general direction of question (subjective or objective)	S–D	A	*Did you see* (a)...	$A>W$	$A>W$	$A>W$
	O–D	W	*Was there* (a)...			
	S–D	C	*Did*(n't) *you see* (a)...	$C>X$	$C>X$	$C \boldsymbol{<} X$
	O–D	X	*Was*(n't) *there* (a)...			

* 'S–D' here signifies 'subjective-direction,' 'O–D' similarly 'objective-direction.'

With this explanation we may proceed to consider the results.

(*a*) *Influence of the article.* It has been seen that the influence of the articles does not appear to be constant when different pairs of question forms are compared. If the caution connected with C were greater than that connected with D, and if a similar relation held between these two forms in respect of reliability, the results would appear as consistent, since these relations would be identical with those between A and B. If the results obtained from a comparison of C and D did correspond with those obtained from a comparison of A and B, we should be able to state the following generalization: *changing the indefinite into the definite article in a question form decreases caution, decreases suggestiveness, and decreases reliability.* Let it be provisionally assumed that this generalization is true and that the anomaly in the case of C and D (caution and reliability) can be explained.

(*b*) *Influence of the negative.* Here also the influence of the specific factor does not appear to be constant when different pairs of question forms are compared. A comparison of A with C and of W with X yields a consistent result; and the results would be entirely consistent if the caution connected with D were less than that connected with B, and if a similar relation held between these two forms in respect of reliability. If the results *were* entirely consistent in this way, we could assert the following generalization, which is in accord with the accepted theory on the subject: *the introduction of a negative into a question form decreases caution, increases suggestiveness, and decreases reliability.* Let it be assumed provisionally that this result is justified, and that the anomaly in the case of B and D (caution and reliability) can be explained.

(*c*) *The anomalies* (*C with D*, and *B with D, caution and reliability*). The difficulty of making the two generalizations provisionally accepted above is connected with one question form especially, namely, D. This is significant in view of the fact that these generalizations have to do with the influence of the articles and the negative respectively, and that D contains *both*. In explanation of the results it might be suggested that the influence of the definite article might be in certain respects opposite to that of the negative; and that, if this were so, anomalous results might be obtained when both were included in the one question form. It will be found, however, that both the conversion of the indefinite into the definite article and the introduction of a negative in a question form have a *similar* influence upon caution and reliability, being opposed only in their influence on suggestiveness (accepting the above two provisional generalizations; that is to say, the comparison of A with B is supposed to yield the *correct* result for the influence of the article on caution and reliability, while the *correct* result for the influence of the negative upon these factors is supposed to be given by a comparison of A with C and of W with X). The difficulty thus occurs in connexion with the very points where the two influences should reinforce each other. Both the negative and the definite article, when used separately in a question form, decrease caution and reliability. We should expect that when used together caution and reliability would be still further decreased. Instead of this they are increased.

The following considerations may be offered in explanation of this fact. Speaking with some looseness, a suggestion is only effective if it is not too obvious. One person, for example, may induce another to agree to a certain course of action by assuming a personal interest in him; but if the professed interest be rather ostentatious, it may overreach itself and perhaps give rise to a contrary suggestion. May not a similar phenomenon occur here? When a negative is introduced into a question form the subject is inclined (according to the second of the above generalizations) to be less cautious, accepting a kind of implication in the resulting form that an assertion of presence is the correct answer, and consequently decreasing the reliability of his answers. Again, by changing the indefinite into the definite article in a question form, a *similar* result (according to the first of the above generalizations) is obtained. But when a negative is introduced into a question form, and the indefinite is changed into the definite article in the same question form, the suggestive implication in the resulting question form may perhaps become *too obvious*, with the result that caution and reliability, instead of being still further decreased, are increased.

If this explanation be correct, the two provisional generalizations made above may be accepted. It will be noted in any case that the part of them dealing with suggestiveness is more or less certain, since the results are throughout consistent concerning the influence of the various factors upon suggestiveness (Table V). As the explanation connects with well-authenticated facts there is considerable evidence in its favour; and we may therefore accept the above two generalizations, in so far as caution and reliability are concerned, as being *probably* true.

The second of these generalizations may appear intelligible enough, as the decreased caution and reliability seem to be effects of the increased suggestiveness; but it may appear that no significance attaches to the generalization concerned with the influence of the articles. What can be meant by the proposition that some factor decreases all three measures, suggestiveness, caution, and reliability? The answer to this question depends chiefly upon the fact that the words suggestiveness, caution, and reliability, are here used somewhat more strictly than in ordinary speech; but it also seems to depend upon an important psychological fact

If Table IV be considered, it will be seen that the proportion of denials of presence returned as answers to questions of type *B* is *very* much larger than that connected with any other subjective-direction question form. It is nearly five times as large as that connected with *A*. Now the *A* questions contained the indefinite article simply. The *B* questions contained the definite article but no negative. In the question of type *B*, *e.g.*, "Did you see the dog?" there appears to be an implication that a dog was 'there' to be seen. It is as if the questioner accepts this as a fact and proceeds (in the question) to ask if it was *seen* by this particular individual. This implication is apparently often felt so strongly that the person questioned is urged to answer *it* rather than the question. If no dog was *seen*, a person is willing to reply to the question of *A* type (Did you see a dog?) with a simple "No"; and he will not wish to add to this statement concerning his perception unless he be *very* sure that no dog was *present*. Even if he judges it unlikely from his memory of the observation material that a dog was present, he will generally be satisfied to reply to the *question*, saying simply that he did not *see* a dog. Suppose, however, that instead of the *A* type of question the *B* type (Did you see *the* dog?) is employed. The person questioned, finding now that the questioner takes it for granted that a dog was present, is roused to assert his own opposite belief (which he may have held, even if somewhat vaguely, when the *A* question was asked, although he was then content to keep the belief to himself).

It may then be said that if a person has not *perceived* the object, etc., enquired about, he is generally content to answer the question of type *A* (Did you *see* a?) literally, that is, with a statement concerning his *perception*; but that if the question be asked in the *B* form (Did you see *the*...?), he is roused to state his belief concerning the objective fact. We thus have an explanation of the relatively large number of denials of presence made in answer to questions of type *B*.

But how is it then, it may be asked, that in connexion with type *D*, which also contains the definite article (Didn't you see the...?), the proportion of denials of presence is relatively small? (see Table IV). The answer probably depends upon the fact that *D* contains both the definite article and the negative, and this combination, as we saw, probably increases caution. Increased caution would be likely to decrease the number of denials of presence, the person who is asked the question "Didn't you see the dog?" feeling, probably, that this form of question *certainly* implies the presence of a dog, and that, even if he does not believe there was one, he had nevertheless better be content with answering literally the question put to him, thus answering simply that he did not *see* a dog.

When we bear in mind that suggestiveness is decreased by an increase in the number of denials of presence as well as by a decrease in the number of assertions of presence, the proposition that changing the indefinite into the definite article in a question form decreases caution, suggestiveness, and reliability, seems to be intelligible. The decrease in *suggestiveness* results from the fact that the proportion of denials of presence is increased while the proportion of assertions of presence remains approximately unaltered (cf. *A* with *B*, Table IV). The decrease in *caution* follows as a consequence, the proportion of 'assertions *plus* denials' of presence being greater for the question form with the definite article than for that with the indefinite article. It is then not difficult to understand that *reliability* also is decreased.

It seems probable that the 'implicative' force of an explicitly defined content would be similar to that of the definite article, that is, it might arouse denials of presence; but any result would obviously depend upon the character of the 'content' also, and upon its relation to the observation material.

(*d*) *Influence of direction of question.* Such general statements as have been made above (§ 9) concerning the relations between subjective-direction and objective-direction question forms were not based upon a comparison of those forms that differed in direction *only*, but upon a comparison of all the subjective-direction with all the objective-direction question forms (exclusive of *Y*), whatever differences other than the direction difference might thus be included. The forms which differ in direction only are *A* and *W*, and *C* and *X*.

Comparing these, it is seen that what was said above concerning the relatively greater caution connected with subjective-direction question forms must here be in principle repeated; and it appears that this difference is a specific effect of difference in direction. Comparing *A* with *W* in respect of caution the figures are 81 and 50; comparing *C* with *X*, 79 and 45. It was shewn above that this difference in caution is made up of (1) caution that is allowed for and (2) caution that is produced (pp. 372–4).

It may at first sight appear strange that a similar relation between subjective-direction and objective-direction question forms holds for suggestiveness. The figures (Table IV) leave no room for doubt with regard to the fact. Comparing *A* with *W* in respect of suggestiveness, the figures are 89·2 and 43·6; comparing C with X, 91·7 and 51·8

With regard to reliability, however, the result does not appear to be entirely unambiguous. Comparing the two pairs of question forms with respect to reliability, $A > W$, but $C < X$. It is true that the reliability of A is very much greater than that of W, while the reliability of C is *not* very much less than that of X. If we look at Table II, however, we see that it contains only one question of A type, and the reliability of this question is zero. If more questions of this type had been used here, the reliability of this type in Table II would certainly have been raised a little; but the greater absolute number of wrong answers then obtained would have had the effect of lowering the total reliability of the A type, so that the difference between A and W in respect of reliability would not have been so great as it is. The reliability of the A form would nevertheless probably have been greater than the reliability of the W form (judging from such figures as we have) if in each case these questions had been equally divided among the two classes (1) those with true content, and (2) those with false content.

Let us therefore provisionally assume that a comparison of A with W yields the correct conclusion of the relation between subjective-direction and objective-direction question forms in respect of reliability. We assume, that is, that changing the direction of a question form from subjective to objective lessens its reliability—although we need not assume that the difference is so great as is suggested by a comparison of A with W. We have then to explain how it comes about that the reliability of C is less than the reliability of X.

It was shewn above (p. 378) that the probable result of the introduction of a negative into a question form is (*inter alia*) to decrease its reliability. We assume *here* that changing a subjective-direction into an objective-direction question form also decreases its reliability. Now X has both the negative and the objective-direction form. It might be expected that these factors would reinforce each other, with the result that the reliability of X would be very low. The opposite result may be due to the possible fact that here, also, the attempt to mislead, as it might be called, overreaches itself. We should thus have a phenomenon analogous to that discussed above (p. 379). There it appeared that, although the influence of the negative, and of the definite article, was, when used separately, to decrease reliability, nevertheless when the two were included in the one question form reliability was increased. A similar explanation seems to be the most likely one here also. We shall therefore state as a *probable* generalization that converting a subjective-direction into an objective-direction question decreases reliability, suggestiveness, and caution. The facts concerning caution and suggestiveness, however, leave room for no doubt. That part of the generalization concerned with them must therefore be regarded as *certain*.

(*e*) *Further remarks concerning reliability.* Since the relative reliability of a question form is of practical importance, some further consideration of the measure of reliability and of the factors of which the reliability is a function seems necessary.

First, the above generalizations are *all* subject to the condition that the number of different questions with true content[1] employed shall be equal to the number of different questions with false content[1]. This condition can be easily satisfied in a laboratory experiment where the correct answer to any question (in the sense of 'correct' used in this paper, see p. 361) is known by the experimenter beforehand. But in life questions are asked because the correct answers to them are not known, and there it is not known beforehand what is the relation of a question's content to objective fact. Thus arises a complication which is of such a character as to prevent the word 'reliability' being used of one question form as compared with another *except under the above-stated condition*.

For the reliability of a question form is a variable factor, and may easily be made to assume different values by a selection of the different questions from the answers of which it is calculated. If, for example, Table I be considered, a comparison of the right and wrong answers shews that the reliability of the subjective-direction questions, if calculated from the figures in this table only, is extremely high; but if, on the other hand, the reliability of the *same* forms be calculated from the figures in Table II only, it is extremely low,—that for *A* being in fact zero (cf. Table VI, and the note to it, p. 386). These reliabilities however are in each instance calculated from a selection of the questions used, the principle of selection being the truth or falsity of the question content.

The divergences in the reliabilities thus obtained are, in the case of subjective-direction questions, a function of (1) suggestiveness, (2) caution, and (3) the relation of the content of the questions to reality. Suggestiveness and caution do not appear to depend upon the truth or falsity of content, since these measures, when calculated from

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[1] See note on p. 360.

Table I, are roughly the same as when calculated from Table II. That is, suggestiveness and caution appear to be direct results of the form of a question (when the *content* is unsuggestive); while *reliability is not simply a function of a question's form,* but of caution, suggestiveness, *and the relation of the question's content to reality.* To compare question forms in respect of reliability, it is therefore absolutely necessary to equalise the content with the utmost strictness; and *assuming such equalisation as regards truth and falsity of the content,* it may be said that the reliability of a question form is a function of that form's suggestiveness and caution.

11. CONCLUSIONS CONCERNING THE SPECIFIC QUESTION FORMS.

The conclusions arrived at above will now be summarised. Those which are certain[1] will be distinguished, by being italicised, from those which appear more or less probable only. 'Reliability' will be used strictly, in accord with the principle laid down for its use in the preceding paragraph.

(1) The article. Changing the indefinite into the definite article in a question form

(*a*) *decreases suggestiveness,*
(*b*) decreases caution,
(*c*) decreases reliability.

(2) The negative. Introducing a negative into a question form

(*a*) *increases suggestiveness,*
(*b*) decreases caution,
(*c*) decreases reliability.

(3) The direction. Changing a subjective-direction into an objective-direction question form

(*a*) *decreases suggestiveness,*
(*b*) *decreases caution,*
(*c*) decreases reliability.

(4) The definite article + a negative. Including both the definite article and a negative in the one question form

(*a*) decreases the suggestiveness of the resulting form compared with that containing the indefinite article and a negative, but increases the suggestiveness of the resulting form compared with that containing the definite article and no negative;

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[1] Conclusions are taken as 'certain' where the results which they generalize are throughout consistent. This happens chiefly with suggestiveness (see Table V).

(*b*) increases the caution, and
(*c*) increases the reliability,

of the resulting form compared either with the form containing the indefinite article and a negative or with that containing the definite article and no negative.

(5) The objective-direction + a negative. Including both the objective-direction and a negative in the one question form

(*a*) *decreases the suggestiveness,*
(*b*) *decreases the caution,* and
(*c*) increases the reliability,

of the resulting form compared with that containing the subjective-direction and a negative.

(6) *Z,* the so-called implicative question form, *is lower than all the other question forms investigated, for suggestiveness, caution, and reliability.*

This question form is: "Was the *K, m?*" *K* was always an object (or objects) or event noticed by the subject. This conclusion therefore suggests that statements as to the occurrence or non-occurrence of an object or event, which generally speaking made up the answers to all the questions except those of *Y* and *Z* forms, are more reliable than statements concerning specific characteristics of such objects or events. The psychological explanation of this fact may be that a certain amount of knowledge possibly produces rashness, the consciousness that an object or event has been observed inducing a feeling of capacity for describing it more or less fully—a feeling which may be quite unjustified.

(7) *Y, the incomplete disjunction question form, possesses a relatively high suggestiveness*[1], *a relatively low caution*[2], *and a relatively low reliability*[2].

(8) General conclusion. The general conclusion from the foregoing is that the most reliable form of question is a subjective-direction question form which contains neither a negative nor the definite article (nor an equivalent of the definite article). If the content of a question be defined with some explicitness, it seems probable that the resulting question will carry an 'implicative' force similar to that carried by the question form containing the definite article (see p. 355), and produce similar results (p. 380).

This conclusion is subject to certain qualifications, the character of which will be discussed in the next section.

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[1] Cf. § 8, (*b*); and pp. 370–1.

[2] The chief factor operative in producing these results may be that mentioned in connection with the *Z* question form. Cf. this section (6).

12. PRACTICAL VALUE OF CONCLUSIONS.

The practical significance of our conclusions may be regarded from the point of view of abstract justice and from that of the *ex parte* counsel.

The important question is which of the question forms investigated in the present experiment is *in practice* the most reliable. The preceding discussion of reliability will render obvious the difficulty of answering this question. We may however exclude such question forms as contain a negative or the definite article (or an equivalent), and also the 'incomplete disjunction' and 'implicative' question forms, as less reliable than others. The problem then becomes whether from the practical point of view of arriving at the truth it is better to employ a subjective-direction or an objective-direction question form (with content not too explicitly defined).

If any finite number of questions that are asked in connexion with any legal case could be divided into two equal classes, those with true and those with false content, the general conclusion stated above (p. 385) with regard to the greater reliability of subjective-direction forms would be established. It is obviously impossible to assume such an hypothesis. The difficulty with which we are in consequence faced may be illustrated by the following table.

TABLE VI.

		A	*W*	C	*X*
Reliability, calculated from questions with *true* content	...	**88**	56	**89**	58
Reliability, calculated from questions with *false* content	...	**0***	76	**5**	56

* This would certainly have been a *small positive* quantity had several questions of this type (with false content) remained after elimination. Only one remained (cf. Table II).

If it be assumed that in a given case the questions asked were of type *A* and that the content of each of them was false, the answers received would be extremely unreliable. There would perhaps be twenty times as many incorrect[1] as correct answers. This would render any ideal reconstruction of the facts exceedingly difficult. It is true that if the content of the question *happened* to be true, their answers would be mostly correct; but in the investigation of the truth the person who asks the question does not know whether its content is false or true, and he is consequently faced with the alternatives that any definite answer given to it is either almost certainly true or almost certainly false.

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[1] For the criterion adopted here of correctness or incorrectness, see p. 360.

Would a better result follow from the employment of objective-direction forms? The right and wrong answers would then be more nearly equal, both when the content of the questions was true and when it was false; and the right answers would probably be slightly the more numerous. In certain cases, and according to the nature of such facts as were known, this might be very important for the ideal reconstruction of the facts; especially as the percentage of right answers for objective-direction questions is greater than that for subjective-direction questions (cf. Tables I–IV). On the other hand, the false answers may mislead. The fact is that while the probability of any answer to an objective-direction question being true *is greater than* the probability of any answer to a subjective-direction question being true, the probability of any answer to an objective-direction question being *false* is also *greater than* the probability of any answer given to a subjective-direction question being false (cf. Tables I–IV). That is to say, the answers to objective-direction questions, while they contain more right answers than do the answers to subjective-direction questions, contain also more wrong answers.

There is therefore little to choose between subjective-direction and objective-direction questions considered as instruments for the discovery of truth. Nevertheless their answers exhibit great differences, and it is in the interest of justice that these differences should be recognized.

Thus, a counsel may wish to discredit in the eyes of a jury some inconvenient witness. This he may do by putting a question so that the witness returns a wrong answer (such as can be shewn to be false), or by convicting the witness of mere rashness. He may, then, ask the witness an objective-direction question; and he will find that the witness can thus be made to assert the occurrence or non-occurrence of something, concerning which he can be made later, by being asked the same question in a subjective-direction form, to assume an "I do not know" attitude. He would thus be made to appear rash. Or, by continually asking a witness subjective-direction questions with false content, he may be readily induced to give incorrect answers (*with very few that are correct*). He would thus be made to appear untrustworthy. To counteract a misuse of such methods, it should be remembered that practically any witness can be made to give answers that are both rash and incorrect.

A reference must be made in conclusion to the probable effect of the "No" answer to a subjective-direction question upon a jury. There is no doubt that in many instances such answers are taken as implying this or that concerning the character of 'objective fact.' Logically no such implication exists. Further, there is, generally speaking, no psychological implication, since objects or events 'within the range' of observation are frequently not perceived. It may be thought that a contrary conclusion may be drawn from Table II, where the "No" answers to subjective-direction questions are in fact correct, since the content of each question is here false: that is to say, subjects *frequently* replied that an object or event was not perceived when in fact it was absent. If Table I be considered, however, it will be seen that subjects almost as frequently replied similarly when the object or event was present. It is therefore clear that no inference is in general possible from the fact that something was not perceived to the conclusion that it did not occur.

Intimately connected with this point is the effect of the "I do not know" answer upon a jury. "I do not know" implies "I did not perceive," just as "I did not perceive" implies "I do not know." Now one of the most important facts which emerge from a consideration of the eliminated questions is that no matter how obvious it might seem that one particular answer to a question was correct, a considerable number of subjects would be found to take up a non-committal attitude, saying "I do not know."

F_1, for instance, was concerned chiefly with two men, a table, and a blank wall as background. The table was placed between the men, so that no confusion with regard to their figures was ever possible. Yet in answer to the question (F_1 1) "Was there a third man anywhere?" 18 *out of* 55 subjects stated that they did not know. Some said that they did not *see* a third man, but that they would not swear to his absence. Such caution with regard to small things may be considered fairly natural; but in the present connexion it is rather remarkable.

This is by no means the only instance, however, in which it occurred. Consider F_1 17. The 'wall' was 'the background'—it was a cloud photographic background. The incident took place immediately before it, so that it was in line with the focus of attention the whole time. Yet 43 out of 54 subjects asserted that they had not seen 'the picture,' *implying* that they did not know if it was there or not. Many said, indeed, that they would not be surprised if it were there. And this, notwithstanding the apparent obviousness of the fact that there was no picture on 'the wall.'

Other similar instances occurred. The important fact is that some witnesses are disinclined to deny the occurrence of almost any object or event, even if the truth with regard to it appear very obvious. Such an attitude on the part of a witness has considerable influence upon a jury. It is thought that if a certain intelligent witness will not swear that such-and-such a thing did *not* occur, it quite probably *did* occur; and the tendency to adopt this conclusion is strengthened if some other witness, who is possibly rash in his judgments, definitely asserts its occurrence. Thus, if three witnesses will not swear that "a sixth man was *not* present" on a certain occasion (when in fact no sixth man was present), while one witness asserts that a sixth man was present, a jury will be very much inclined to believe the incorrect statement. It should therefore be recognized that, unless the objects, etc., enquired about are such as to render their presence or absence very extraordinary, the disinclination of certain witnesses (who are often highly intelligent) to swear that they did not occur is no evidence that they did occur. As a matter of fact it frequently happens under such circumstances that the events referred to did not occur.

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PUBLICATIONS RECENTLY RECEIVED.

Anthropomorphism and Science. By OLIVE A. WHEELER. London: George Allen Unwin, Ltd., 1916. *5s.* net.

Miss Wheeler has given a very vigorous exposition of what she terms ejective cognition. The title of her book indicates its main purpose which is to show how considerable an amount of the knowledge we ordinarily describe as scientific is ejective in its origin. Starting from the postulate that we can have no direct knowledge of other minds she makes it her task to reconstruct the genetic process by which this belief is obtained and established. The argument falls into two main divisions, one dealing with the process in the individual, the other with the process in the race. Although it is only in regard to the belief in other minds than our own that Miss Wheeler demands our assent to her postulate that these objects can only be known indirectly, the process by which the belief is established is not confined to this special class of objects; it is, she holds, a process at work in cognition generally.

The book bears evidence of wide reading; and the author has made herself familiar with the most recent advances in scientific and philosophical theory. She attacks or defends one theory after another with unwavering confidence in the truth of her thesis. We are sorry however that she has confined her energy entirely, as it seems to us, to theoretical arguments, her practical and experimental illustrations being taken straight away from standard works. A first hand study by her of this genetic problem would have been most valuable, whether or not it would have affected her confidence in the truth of her theory. Miss Wheeler begins by telling us that no sane man is a solipsist. So far as I can make out from Miss Wheeler the insanity of the solipsist consists in his having brought himself to believe what he knows to be true, whereas everyone else manages to believe what he knows is not true. Indeed it seems that all of us who are sane owe our sanity to the extraordinary perversity we exhibited in the early period of our infancy. A recent psychological theory attributes all the disorders, and they are innumerable, of adult life, to repressed infantile wishes struggling to find some fulfilment. This is nothing to the havoc wrought in our intellectual life by the logical processes which some psychologists suppose must take place in the infant consciousness. The chief of these is what Miss Wheeler calls ejection. The infant knows, or at least could know, its own consciousness, but clearly does not and could not know any but its own. What does this infant at once proceed to do? It ejects its consciousness into all the objects of its cognition without exception, into spoons, plates, tables, chairs, dogs, cats, dolls, bunches of straw, everything, and then as its growing experience makes its theory untenable, it retracts it, but only slowly and by degrees, till the "sane" belief is left, *i.e.* the ejected consciousness is restricted to bodies with a certain organic resemblance to its own. And this is how we have all come to our belief in other peoples' minds!

However ridiculous this may sound, I do not want to suggest that therefore it is not true. I am quite willing to admit that if I must accept the postulate that there is no direct acquaintance with other minds but that all knowledge of such is inferred, the result of an actual logical argument, then any representation of that logical argument taking place, as apparently it must have done, in the consciousness of "the baby new to earth and sky" must seem somewhat comical. At any rate no absurdity attaches to Miss Wheeler's treatment of the problem which is not equally chargeable to the account of the many recognized authorities she follows. All that

I want to direct attention to is the postulate itself, for if the absurdity of that postulate can be demonstrated there is clear gain. And I see a sign of grace even in Miss Wheeler's account. Timidly and apparently with some misgiving she admits (p. 117) that there may be another mode of knowing than that of the intellect, an instinctive mode revealed in "sympathy." But if this means, she hastens to add, that we can be supposed to know "we" before we know "me," she will have none of it.

Miss Wheeler's confidence makes her write with clearness and force. She well deserves the M.Sc. awarded to her. We hope she will be encouraged to pursue her studies and undertake for a doctorate some original psychological research.

H. W. C.

Normal and Abnormal Psychology. By Boris Sidis, A.M., Ph.D., M.D. London: Duckworth and Company, 1914. *7s. 6d.* net.

This volume forms an attempt to formulate the fundamental assumptions that underlie normal and abnormal psychology. It comprises a preface, fifty-four chapters, and two appendices.

The preface consists chiefly of a vigorous polemic against all forms of applied psychology, except the medical. "Applied psychology," it is maintained, "is nothing but a nightmare." "We can," however, "obtain some help from abnormal psychology in its application to the medical treatment of nervous and mental maladies. This is quite natural, as abnormal psychology is essentially based on clinical and experimental [study?[1]] of mental diseases. The claim, however, that psychology can give directions for vocations of life or for business and industry is entirely unfounded. The same holds true of the practical pseudo-psychology that has invaded the school, the court, the prison and the immigration bureau. The intelligence tests are silly, pedantic, absurd, and grossly misleading."

The body of the work is divided into two parts. The first deals generally with 'fundamental concepts and principles.' It commences with a discussion of the definition, objects, scope, and sources of psychology; and then emphasizes the importance of pathological psychology for psychology as a whole. "In the investigation of mental life we may change either the objective content, or in the mental function itself." "In the study of memory," for instance, "we may follow the method of Ebbinghaus,...analyzing the changes effected in its contents; or we may study the mechanism of memory by studying its disturbances in different forms of diseases. The latter is by far the more valuable....In psychology, the pathological method is by far the most important."

A section follows upon 'the fundamental hypothesis of psychology.' In a series of chapters, the familiar hypotheses are discussed—'spiritualistic,' 'materialistic,' 'faculty,' 'transmission,' 'psycho-physiological.' Finally, a 'positive' as distinguished from a 'metaphysical' form of the 'psycho-physiological hypothesis' is accepted. A suggestive little chapter is inserted upon 'the chance aspect of life and mind.' "Chance thoughts, meaningless images and ideas, like accidental variations, form one of the most important factors in the evolution of purposive mental activity." From this standpoint the doctrines of Freud and the psychoanalysts are briefly criticised. "Our dreams, our unintentional errors in speech, writing and action are due to the many chance thoughts which either intrude themselves on consciousness in spite of the selective rigid process of attention, or are due to the momentary relaxation of the selective process." "The so-called 'psychoanalytic science' is erroneous, not only because of its fallacious 'psychic causation,' but also because it is based on the fallacy of regarding each and every mental state as purposive in character. This pseudo-psychology misses the fundamental fact that many psychic occurrences are like many biological occurrences, mere *chance variations....Not purpose, but chance is at the heart of mental life*" (author's italics).

There follows a series of chapters, first, upon the nature of perception, and, secondly, upon the functions of the sub-conscious. The former, for the most part, formulate acknowledged doctrines in a clear and suggestive fashion. The bearing of familiar distinctions upon pathological states is often put with sweeping vigour and freshness. "Functional psychosis with all its protean manifestations, may be reduced to variations of one fundamental attribute—vividness....Dissociation varies inversely as vividness. When vividness is so low as to reach a minimum,... there is a break, a gap in mental continuity; dissociation results." The chapters on the sub-conscious largely re-state, with modifications, the doctrines which the author has already made familiar in various books and papers. Part I closes with a chapter on 'reserve energy.' The writer develops from a somewhat different standpoint the view familiarised by James. "In the study of functional nervous and mental diseases, one becomes more and more impressed with the fact that beyond the psycho-physiological limits of energy, available for the habitual adjustments to the ordinary external conditions of life, there is a vast store of reserve energy whose depths one cannot gauge."

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[1] The substantive is omitted in the original, presumably by a printer's error.

Part II deals at length with the theory of 'Moment Consciousness',—a theory already outlined by the author in his *Psychology of Suggestion*. "A series of psychic events must exist in and for some psychic unity or individuality which stands for the organic unity of consciousness, no matter what the type of consciousness is, low or high, animal or human. The synthetic unity of consciousness, no longer a series, is indicated by the term 'moment' or 'moment consciousness.' There are various types of moment consciousness, according as there are various types of synthesis." The three types of moment-consciousness are discriminated: (1) 'the desultory moment,' (2) 'the synthetic moment,' (3) 'the recognitive moment.' In the 'desultory moment' the 'psychic' links are not interconnected. In the 'synthetic moment' each link related to and synthesises preceding ones. In the 'recognitive moment' "the content of the previous occurrence need not be actually reproduced, but only represented." The various types, and their sub-types, are then described in detail as they are conceived to appear in normal and pathological states.

The first appendix contains a criticism of James's view of the non-existence of consciousness and McDougall's view of psychology as the science of behaviour. The second appendix briefly expounds T. B. Robertson's hypothesis that nervous facilitation,—the deposit of neural 'traces,'—consists in an auto-catalytic reaction, *i.e.*, in a chemical reaction in which one of the products accelerates the reaction.

This summary and these extracts cannot do justice to the comprehensive character of the author's survey. It will, however, be seen that his aim is not so much to present new facts or new hypotheses, but rather to re-state familiar and accepted principles in a fresh light and from an original standpoint.

School Discipline. By W. C. BAGLEY. New York: The Macmillan Company, 1915. 5*s.* 6*d.* net.

This book deals with a problem that is largely psychological in character, namely, the methods of creating in an unruly school a spirit of good discipline. It is a practical textbook, written with the needs of the young teacher primarily in view.

The psychological characteristics of a well-disciplined school are first described. Silence, rigid postures, precise movements are relatively inessential. The essential thing is 'a fashion of good order.' Discipline, according to modern ideals, is a mental rather than a physical attitude. The directive force is not the will of the master so much as the collective will of the school or class.

The unruly school is discussed at greater length. Among its specific causes the psychological characteristics of the teacher's personality are given chief prominence: lack of sympathy, vacillation and weak will, procrastination, ungoverned temper, tactlessness, and inability to grasp the pupil's point of view.

To transform the unruly school, the following methods are recommended and analysed: cultivating an objective attitude, raising the standard of school work, assigning special problems to special individuals, stimulating group responsibility. Direct coercion is allowed only as a supplementary means.

Troublesome children are classified into seven or eight types; the psychological characteristics of each are enumerated; and their pedagogical treatment outlined. The classification tentatively suggested is (1) the haughty pupil, (2) the stubborn pupil, (3) the self-complacent pupil, (4) the irresponsible pupil, (5) the morose pupil, (6) hypersensitive, (7) the deceitful pupil, (8) the vicious pupil. In connection with the last, there is a plea for fuller investigation of the sex impulse as a preliminary to the recommendation of effective measures for sex-hygiene.

The author concludes with a chapter on the relation of discipline to interest and mental growth.

The book is not only a most suggestive handbook for the inexperienced teacher. It is replete with problems for psychological research upon the nature of individual character and the interaction between personalities. As the writer observes, the nature of the 'troublesome types' in itself furnishes a rich field for intensive enquiry.

Inventors and Money-makers: Lectures on some Relations between Economics and Psychology. By F. W. TAUSSIG, Ph.D., LL.B., Litt.D. New York: The Macmillan Company, 1915. 4*s.* 6*d.* net.

The lectures deal chiefly with certain human instincts, considered as direct springs of industrial activity. The author discusses and rejects the assumption of a specific instinct to trade—'the propensity to truck, barter or exchange,' in the phrase of Adam Smith. From a list based partly upon the writings of James, McDougall and Graham Wallas, and partly upon popular speech, he selects the 'instincts' of contrivance or construction, of acquisition or collection, of domination or pugnacity, of emulation and of imitation. From these he derives that complex phenomenon, the desire for wealth. The 'instinct' of devotion is discussed in detail: and early economic psychology criticised at length for ignoring the influence of inborn altruistic tendencies upon human activity and work.

Mind, No. 99. London: Macmillan and Co., 1916. 4*s.*

The Philosophical Review, Vol. XXV, Nos. 3 (Special Number: 'Papers in Honor of Josiah Royce on his Sixtieth Birthday') and 4. New York: Longmans, Green and Co., 1916. 3*s.* net. Special Number, $1.50, No. 4, 60*c.*

CORRECTION.

On p. 178, *Vol. VIII, Part* 2, *in Table VI C of article on* 'Factors in the Mental Processes of School Children,'

for '+ *l*' *read* '+ 1' throughout.

N. C.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

May 13, 1916. An Experimental Study of Some Problems of Perceiving and Imaging. By F. C. BARTLETT.

The Involuntary Nervous System and the Involuntary Expression of the Emotions. By JESSIE MURRAY.

VOLUME VIII JUNE, 1917 PART 4

UNIVERSITY OF CALCUTTA LIBRARY THE ADVANCEMENT OF LEARNING

ON THE DIFFERENTIATION OF THE HUMAN FROM THE ANTHROPOID MIND.

BY CARVETH READ.

1. *Heredity, Adaptation, Accommodation.*
2. *The Original Stock and the Conditions of Differentiation.*
3. *Primal Society.*
4. *The Psychology of the Hunting Pack.*
5. *The Wolf-type of Man established by Natural Selection.*
6. *Some further consequences of the Hunting Life: Language, Customs, Property, Benevolence, War.*
7. *Influence of the Imaginary Environment.*

1. HEREDITY, ADAPTATION, ACCOMMODATION.

FOLLOWING the general belief that Man is descended from a stock nearly allied to the greater anthropoids—Orang, Chimpanzee, Gorilla—we may assume that his mental endowments were once much the same as theirs; and that, so far as they are still the same, (1) heredity sufficiently explains his having them. Thus the senses, perception, the simpler forms of comparison and inference, the appetites and many of the instincts and emotions are common to us with the apes, are seen in our children under three years old, and (in short) constitute that generic consciousness (as I have called it) from which the human mind in general and the peculiar traits of races and individuals are differentiated.

So much for heredity; but the differences of the human from the anthropoid mind, alike in intelligence and in character, are enormous, and must be accounted for in some other way. Allowing for some original specific difference which we can hardly hope to discover, the changes that have taken place may be considered as the result of (2) adaptation to those habits of life under which our species (now ranking zoologically

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as a family) has been developed. And this adaptation I shall assume to have been brought about under conditions of natural selection: human races, as we now see them, being the survivors of many variations, more or less successful, and the others having been destroyed. For good judges are of opinion that, amongst the discovered remains of ancient specimens of the human family, some that exhibit marked deviations from the modern type—Neanderthal, Eoanthropus, Pithecanthropus—should be regarded not as belonging to our ancestral line, but rather as representing distinct species that have failed in the struggle for existence[1].

But besides the innate dispositions of human nature determined by heredity and natural selection, which are found in some measure universally, because they are adaptations to conditions that, at one time and not long ago, weighed upon the ancestors of all of us, there are numerous traits (some of them quite superficial) that vary from country to country and from age to age, according to the economic or political type of the society in which a man lives, his place therein, geographical circumstances, religious institutions and the countless causes that govern manners and customs. In the lives of most men these traits are not necessary; they may be adopted and cast aside more than once in an individual's career: they are temporary (3) accommodations due to education, imitation, tradition; and, in fact, are often the disguises of human nature. Still, as society grows more and more complex, orderly and stable, there is, no doubt, again some natural selection of those individuals who are capable of undergoing the requisite accommodations. Those that cannot endure the restraints of civilization, wander away; the extremely lazy, improvident, dishonest, or aggressive, in considerable numbers, perish.

2. THE ORIGINAL STOCK AND THE CONDITIONS OF DIFFERENTIATION.

To the original mentality of man we can only seek a clue in the higher primates, and especially in the extant anthropoids. No doubt, during the long millennia that have elapsed since the separation of our own stock from those of other genera and species, they also have undergone some evolution, but probably much less change than we have. Unfortunately, our knowledge of their habits and abilities is still deplorably limited. It seems certain, however, that their intelligence is much greater than that of any other kind of animal. They must have extensive knowledge of their habitat, of all the forest can yield for food or shelter, and of its other denizens dangerous or otherwise. They construct for themselves some sort of sleeping place, not much inferior to the Australians' 'lean-to,' by piling branches together in the trees. Toward men, anthropoids seem to be unaggressive, and usually retreat from them; but, when attacked, defend themselves with fury. From other animals the male gorilla has nothing to fear, and he defends his family against leopards; the chimpanzee is said to fight leopards with varying success; and, as for the orang, Dyak chiefs told Wallace that no animals dare attack him, except crocodiles and pythons, and that he kills both of them[1]. The food of these apes is chiefly fruit and the tender shoots of trees and bamboos; but they sometimes eat eggs and young birds; and the gorilla is said to eat small mammals: in confinement they all take cooked flesh freely. Socially, they hardly get beyond family life. Orangs male and female are even seen alone, and young ones together without parents; gorillas are seen in family parties; chimpanzees in families, and occasionally three or four families in company. It is said that gorillas and chimpanzees have been seen together in a large band. I have met with no report of these animals fighting amongst themselves, except that male gorillas sometimes fight for a wife. Gorillas have also been said, upon very slight evidence, to be polygamous; chimpanzees and orangs seem to be monogamous. Their family life is probably, as amongst all the other primates, affectionate: the long youth of their children implies much parental care. Whilst the smaller anthropoids—siamang and gibbon—go in troops, as also do the baboons and most monkeys of both hemispheres, the less sociability of the great anthropoids may be understood to result (*a*) from the limited supply of the right sort of food for them, even in the tropical forest to which they are confined—since animals of their bulk must consume a great deal; and (*b*) from their having no need of combining for the purpose of defence.

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[1] See *The Antiquity of Man*, by Dr Arthur Keith.

From the type thus outlined the mentality of the human race has departed so widely that some even of those who believe that our bodies have been derived from some Simian stock (*e.g.* Wallace) hesitate to admit that our minds can have had a similar history. But as everywhere else in the animal kingdom mind and body constitute one organism, it is reasonable to consider whether the differentiation of the mind of man may not be understood to have taken place under the same conditions as those which determined the transformation of his body. What were these conditions?

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[1] *Malay Archipelago*, 46–7.

(*a*) In *Man*[1] (Nov. 1914) I collected a number of facts and arguments pointing to the probability that the chief cause of the evolution of the human family was the adoption by some anthropoid (or allied form) of the life of the hunter in order to obtain animal food. That the change from a frugivorous to a carnivorous diet may itself have had some effect upon our temperament and activity is possible; but I lay no stress upon that. Most monkeys are almost exclusively frugivorous; the only primate, except man, that depends a good deal upon animal food is, I believe, the crab-eating Macaque (*Macacus cynomolgus*), of the Burmese littoral; yet monkeys are the most alert and active of animals; and some of the most warlike races of men are practically vegetarian. A carnivorous diet alone would not explain any changes in the shape and proportions of our trunk and limbs, nor the upright gait, nor the gregarious habit, nor the development of the brain, nor the invention of weapons, nor the use of fire, nor any of the mental and emotional characteristics that distinguish man from the other primates; but all these things readily follow from our remote ancestor's adoption of the life of the hunter[2]. (For the physical changes I must refer to the article in *Man*.)

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[1] A summary of this article appears in the Report of the British Association, 1913. Lest it should be supposed that recent deplorable events suggested the present paper, I must point out that all its fundamental ideas may be found in the article in *Man*, which was read at Birmingham in September, 1913.

[2] My friend Mr Thomas Whittaker has sent me the following extract from Comte's *Politique Positive*, I. 604–5; "L'obligation de se nourrir d'une proie qu'il faut atteindre et vaincre, perfectionne à la fois tous les attributs animaux, tant intérieurs qu'extérieurs. Son influence envers les sens et les muscles est trop évidente pour exiger ici aucun examen. Par sa réaction habituelle sur les plus hautes fonctions du cerveau, elle développe également l'intelligence et l'activité, dont le premier essor lui est toujours dû, même chez notre espèce. A tous ces titres, cette nécessité modifie aussi les races qui en sont victimes, d'après les efforts moins énergiques, mais plus continus, qu'elle y provoque pour leur défense. Dans les deux cas, et surtout quant à l'attaque, elle détermine même les prémières habitudes de coopération active, au moins temporaire. Bornées à la simple famille chez les espèces insociables, ces lignes peuvent ailleurs embrasser quelquefois de nombreuses troupes. Ainsi commencent, parmi les animaux, des impulsions et des aptitudes qui ne pouvaient se développer que d'après la continuité propre à la race la plus sociable et la plus intelligente. Enfin, la condition carnassière doit aussi être appreciée dans sa réaction organique. Une plus forte excitation, une digestion moins laborieuse et plus rapide, une assimilation plus complète produisant un sang plus stimulant: telles sont ses propriétés physiologiques. Toutes concourent à développer les fonctions supérieures, soit en augmentant l'énergie de leurs organes, soit en procurant plus de temps pour leur exercice."

Sociologists, surveying extant peoples, have usually distinguished four stages of culture, the hunting, pastoral, agricultural and manufacturing; and some have indicated what they suppose to have been a still earlier stage, the 'collecting,' such as may be seen, *e.g.*, amongst the Fuegians. But the collecting state is plainly degenerate, the resource of tribes fallen into distress; it cannot have been the first stage, because it implies no conditions that tend in any way to develop body or mind or society. That hunting came first is a true intuition: and, to understand the development of human nature, we need only refer the hunting life back to the very origin of the human stock.

(*b*) The great anthropoids are all confined to the equatorial forests; and it is obvious that, with their diet, it is impossible to pass out of tropical or (at furthest) sub-tropical regions. But the adoption of a flesh diet enabled the human stock to extend the range of its hunting (allowing for gradual adaptation to climate or accommodation by clothing) to any country that supplied the requisite prey; and, accordingly, in course of time, it wandered to every part of the world. The settling of various off-shoots of the original stock in certain regions long enough for them to undergo adaptation to local conditions is the simplest explanation of existing races: the Negro adapted to equatorial Africa, the Mongolian[1] to Central Asia; the Mediterranean race to the neighbourhood of the sea after which it is named. As for the Nordic sub-race with its fair hair and pale skin, it has the character of an Arctic beast of prey, like the Polar bear: the snow-leopard of the Himalayas is found at an imperfect stage of such adaptation. Some geologists and zoologists now believe that, during the Glacial Period, the climate of North Europe was not such as necessarily to destroy the local fauna and flora; and, in that case, our own ancestors may for ages have maintained themselves there: or if that was impossible (as the scarcity of palaeolithic remains in Scandinavia seems to indicate), they may have roamed for ages along the borders of glaciation, perhaps as far as the Pacific shore. That the race was formerly fairer than it is now is indicated by the whiteness of their children's hair: the trait has outlived its utility. Other races seem to be derivative from these three, as Amerinds chiefly from the Asiatic race; or else to be mixed breeds —the commonest case of all; or, possibly, to represent still older stocks —Australians and Pygmies, for example.

(*c*) Whilst none of the great anthropoids has advanced socially beyond family life, man is everywhere (with few and doubtful exceptions) gregarious—living at the lowest grade in tribes or bands of about fifty; and the gregarious life is one of the most important conditions of his peculiar development. Possibly, he may originally have been more gregarious than any extant anthropoid, in spite of his not needing society for defence, and of its seeming to be for a frugivorous animal inconvenient in relation to nutrition. Moreover, if the great anthropoids and our own ancestors were descended from some lower monkey stock, such as always goes in a troop, the gregarious instinct may have remained with them as a latent character. Still, it is my conjecture that man became gregarious, or recovered the social habit, because of the utility of co-operative hunting; so that he became at first a sort of wolf-ape. This will be discussed in the next section. I observe here, however, that the hypothesis helps us to understand why man is still imperfectly sociable; the purpose of the hunting-pack, each wolf-ape seeking prey, was unfavourable to social life in other relations. Current speculations about fashion, imitation, tradition, crowd-psychology, seem to me in danger of exaggeration, and to overlook the patent facts of individualism, as shown by the hypocrite, the criminal, the vagrant, the contra-suggestible, the hermit, the sceptic, and others. Some people—without being in any way morbid—find that a good deal of solitude is necessary to the complete life.

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[1] Using 'Mongolian' in a generalised sense for the Asiatic stock.

(*d*) The later stages of human development have been considerably modified by certain imaginary conditions peculiar to man; for he—we know not at what date—invented them. These may be summed up under the names of magic and animism.

The chief conditions, then, to which man has been adapted, and thereby differentiated in body and mind from the anthropoid stock, I take to be four: the hunting life; geographical circumstances; social life; and his own imaginations.

3. PRIMAL SOCIETY.

In looking for the probable form of the earliest human or (rather) prehuman society, one naturally makes a survey of other mammalian societies; and the task is soon accomplished. It is surprising how few and simple the types of them are, in contrast with the elaborate polities of some hymenoptera and termites: these have much greater superficial resemblance to modern human societies; but, in fact, they are families rather than societies; their individuals are determined in function not by choice but wholly by structure; and in every way they are too remote from us for any useful comparison. As for mammalian societies, even using the term to include families, they may be classified under four or five types:

(1) Families: (*a*) Monogamous: of which the best example seems to be the chimpanzee. Many of the cats are believed to pair monogamously; but it is doubtful whether, or in what measure, the male takes part in the rearing of the whelps.

(*b*) Polygamous: characteristic of many species of deer;—after the breeding season, the stags often wander away by themselves.

(2) Associations of families without apparent structure or organization, such as those of the vizchaca and the beaver. They have no leaders, and make no attempt at mutual defence; but their incoordinated activities, in making their burrows, dams, etc., have results which, especially in the case of the beavers, look as if the animals had worked upon a common, premeditated plan. Gregariousness exists widely in the animal kingdom without any utility in attack or defence, but merely for convenience of breeding, or for the advantage of signalling the approach of danger, from any direction, to the whole flock.

(3) Troops or herds, comprising several families. This type is common amongst monkeys. Generally the families are monogamous, and both parents care for the offspring; they have leaders, and combine in mutual defence. This is especially effective with the baboons—who, however, are polygamous. A very similar type is characteristic of cattle; who also have leaders as the result of battle between the bulls, each trying to control and keep together as many cows as he can; and they often combine their forces against beasts of prey.

(4) Hunting packs—most noticeable with wolves and wild dogs: they have leaders, and probably an order of precedence determined by battle. In the breeding season (February to August) a pack of wolves breaks up into pairs; but whether their pairing is for life or merely seasonal is disputed; and it is also doubtful whether the male takes any share in caring for the puppies; such habits may vary in different localities. The numbers of the pack depend on circumstances, and are much smaller in Canada than in Russia.

Was our own primitive society, then, like any of these? Since direct evidence cannot be obtained, we must be guided in forming our hypothesis by two considerations: (*a*) what type of society gives the best explanation of human nature as we now find it? and (*b*) for which type can we give the best reason why it should have been adopted? So I point out (*a*) that man, in character, is more like a wolf or dog than he is like any other animal; and (*b*) that for the forming of a

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pack there is a clear motive in the need of animal food, which might be best obtained by co-operative hunting.

It must be admitted that Darwin, discussing sexual selection in man, suggests a different hypothesis. He says: "Looking far enough back in the stream of time, and judging from the social habits of man as he now exists, the most probable view is that he aboriginally lived in small communities, each with a single wife, or if powerful with several, whom he jealously guarded against all other men. Or he may not have been a social animal, and yet have lived with several wives, like the gorilla; for all the natives 'agree that but one adult male is seen in a band; when the young male grows up, a contest takes place for mastery, and the strongest, by killing and driving out the others, establishes himself as the head of the community.' The younger males, being thus expelled and wandering about, would, when at last successful in finding a partner, prevent too close interbreeding within the limits of the same family[1]." The information concerning the polygamy of the gorilla, quoted here from Dr Savage, who wrote in 1845, has not since (I believe) been confirmed.

Naturally, the above passage has attracted the attention of anthropologists; and I am sorry to expose myself to the charge of immodesty in venturing to put forward a different view. Atkinson in his essay on *Primal Law,* edited with qualified approval by Andrew Lang, starts from Darwin's hypothesis, and merely modifies it by urging that the young males, when driven off by their father, did not wander away, but kept near the family, always on the watch to murder their father. This amendment he makes, because he had observed the same habits in cattle and horses. Then, through a row of hypotheses with little evidence or rational connection, he arrives at an explanation of certain savage laws of avoidance, exogamy, etc. More recently, Professor Freud has produced a most ingenious and entertaining essay on *Totem und Tabu,* in which he builds upon the same foundations. You easily see how the 'Oedipus complex' emerges from such a primitive state of things, but will hardly, without reading the work, imagine the wealth of speculation it contains or its literary attraction. Atkinson probably relied upon the supposed parallel case of wild cattle and horses, because those animals resemble the apes in being vegetarian: though the diets are, in fact, very different. But even if such a comparison indicates a possible social state of our original ape-like stock, what is there in such a state that can be supposed to have introduced the changes that made our forebears no longer ape-like? Supposing those changes to have already taken place, what evidence is there that the same social state endured? None: for it was assumed to have been the social state of our forebears on the ground of their resemblance in diet and family economy to the gorilla.

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[1] *Descent of Man,* c. 20.

Returning, then, to the hypothesis as to the chief cause of human differentiation, namely, that a certain primate, more nearly allied to the anthropoids than to any other, became carnivorous and adopted the life of a hunter, there are two ways in which this may have happened: either by such a variation on the part of our ancestor that he felt a stronger appetite for animal food than the gorilla does—strong enough to make him hunt for prey; or by such a change of climate in the region he inhabited—say from sub-tropical to temperate—as to make his former diet scarce, especially in winter, so that he became a hunter to avoid starvation. Everyone admits that he became a hunter at some time: why not at the earliest? Nothing less than some great change of life, concentrating all his powers and straining every faculty, can possibly account for the enormous differentiation of man. The adoption of the hunting life is such a change; and the further back we put it, the better it explains the other changes that have occurred in our physical and mental nature.

From the outset, again, our ancestor may have attacked big game, probably Ungulates—to whom he owed much; for not only did they provide prey, but by clearing the forest over wide areas compelled him to run in pursuit remote from his native trees, thus giving great selective advantage to every variation of legs and feet adapted to running: though at the very first there may have been little need to run, as he was not yet an object of terror; "we must remember that if man was unskilful, animals were unsuspicious[1]." By attacking big game advantage was given to those individuals and families who co-operated in hunting: thus forming the primal society of the human stock; a society entirely different from that of any of the primates, or of cattle, and most like that of the dogs and wolves—a hunting pack[2].

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[1] Avebury, *Prehistoric Times*, 7th ed., 580.

[2] Upon this point, the size of the game first attacked, I have changed my mind since writing the article in *Man*. I then supposed our ancestor to have begun with small animals, learned to make weapons and snares in dealing with them, and then used such implements against larger prey. But it must have been long after the beginning of the hunting life that he first made any but the crudest weapons. Why should he not, at the first, have fallen to with hands and teeth, combining with others in a hungry, savage onset? Later the crudest weapons—sticks and stones—when resorted to, were good against any antagonist.

As in the course of generations the hunting pack developed, no doubt, it had recognised leaders, the most powerful males, one perhaps preeminent. But it was not subject to one old male who claimed all the females; for the more adult males it comprised, the stronger it was; and, for the same reason, pairing, as among wolves, was the most efficient form of sexual relationship. But, in my judgment, it is altogether vain to try to deduce from this form of society, which may have existed 2,000,000 years ago, any of the known customs of savages concerning marriage, such as avoidance, totemism, exogamy; which would be of comparatively recent date if we put back their origin 300,000 years. Many such rules can only have arisen when there was already a tradition and a language capable of expressing relationships.

4. Psychology of the Hunting Pack.

Possibly, as I have said, our ape-like ancestor was more sociable than any of the anthropoids; but sociability in ape-life would in no way account for our present character as men: nothing accounts for it, except the early formation of the hunting pack. Since, however, we can know nothing of that institution directly, we must try to learn something about it from the parallel case of dogs and wolves. Galton remarks how readily the proceedings of man and dog "are intelligible to one another. Every whine or bark of the dog, each of his fawning, savage, or timorous movements is the exact counterpart of what would have been the man's behaviour, had he felt similar emotions. As the man understands the thoughts of the dog, so the dog understands the thoughts of the man, by attending to his natural voice, his countenance, and his actions[1]." No more, if as much, could be said of the terms upon which we stand with a tame chimpanzee, in spite of greater physical and facial resemblance and nearer kinship. What can connect us so closely in mind with an animal so remote from us in lineage and anatomy as the dog is? Adaptation to the same social conditions, the life of the hunting pack.

(1) The master-interest of every member of the pack lies in the chase, because success in it is necessary to life. To show how this passion actuates ourselves, I quote Mr F. C. Selous; who, during an expedition in Canada, roused a caribou stag within twenty yards, saw "the dreadful terror" in his eyes, and shot him. "Did I feel sorry for what I had done, it may be asked. Well! no, I did not. Ten thousand years of superficial and unsatisfying civilization have not altered the

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[1] *Inquiries into Human Faculty*, 262.

fundamental nature of man, and the successful hunter of to-day becomes a primeval savage, remorseless, triumphant, full of a wild, exultant joy, which none but those who have lived in the wilderness, and depended on their success as hunters for their daily food, can ever know or comprehend[1]." To the hunter my paradox must seem a truism. Again, since the interest of the chase culminates in the kill—for this is the condition of making a meal—to kill becomes, in some predatory animals, a passion that is often gratified without regard to their needs. Wolves often slay many more sheep than they devour: a sheep-dog that undergoes reversion kills by night the sheep on neighbouring farms without any call of hunger; and, says Mr Thompson Seton, "the mania for killing that is seen in so many white men, is evidently a relic of savagery; for all these Indians and half-breeds are full of it[2]." They fired at everything they saw. The manners of my own pack—now long dispersed—were very similar to the Indians'; and the sport of pigeon- or of pheasant-shooting has been reduced to its last element—skilful slaying.

(2) The gregariousness of the pack is variable; probably, amongst wolves, it was much greater anciently than it is to-day. There are conflicting statements about the gregariousness of wolves that have been studied in different countries. Couteulx de Canteleu says: "The wolf is an enemy of all society; when they assemble it is not a pacific society, but a band of brigands[3]." Thompson Seton says: "Wolves are the most sociable of beasts of prey; they arrange to render one another assistance. A pack seems to be an association of personal acquaintances, and would resent the presence of a total stranger[4]." Gregariousness of wolves must be reduced by failure of game-supply (as by the destruction of bison in N. America), and still more by the encroachments of civilization (as in France). The primitive human pack, probably, was more constantly gregarious than wolves are: (*a*) because its individuals, having no instinctive or traditionary knowledge of hunting, were more dependent on co-operation; and (*b*) because the long youth of children made it necessary for parents to associate with the pack during their nurture—else no pack could have existed; for whilst wolves are nearly full-grown at eighteen months, apes are not until they are eight or nine years old. At a later period, after the invention of effective weapons, an individual became, for many kinds

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[1] *Hunting Trips in North America*, 349.

[2] *The Arctic Prairies*, 20.

[3] *La Chase du Loup*, 21.

[4] *Life Histories of Northern Animals*, 755.

of game, less dependent on co-operation; but by that time, the hunting grounds of a pack were circumscribed by those of other hostile packs; so that no one dared go far alone.

(3) With gregariousness went, of course, (*a*) perceptive sympathy—every animal read instantly in the behaviour of others their feelings and impulses; (*b*) contagious sympathy—the impulses of any animal, expressed in its behaviour, spread rapidly to all the rest; and (*c*) effective sympathy, so far (at least) as that all united to defend any associate against aggression from outside the pack.

(4) The pack has a disposition to aggression upon every sort of animal outside the pack, either as prey or as a competitor for prey: limited no doubt by what we should call considerations of prudence or utility; which must vary with the size of the pack, the prowess of its individuals, the possession of weapons, etc. After the invention of weapons, many savage tribes can kill every sort of animal in their habitat, as the palaeolithic Europeans did many thousands of years ago. From the outset the human pack must have come into competition with the true carnivores, must have defended itself against them, and discovered that attack was the safest defence. Mr G. P. Sanderson writes: "It is universally believed by the natives (of South India) that the tiger is occasionally killed by packs of wild dogs....From what I have seen of their style of hunting, and of their power of tearing and lacerating, I think there can be no doubt of their ability to kill a tiger.... Causes of hostility may occasionally arise between the tiger and wild dogs through attempted interference with each other's prey[1]."

(5) A hunting pack, probably, always claims a certain territory. This is the first ground of the sense of property, so strongly shown by domestic dogs: the territorial claims of the half-wild dogs of Constantinople are well known. To nourish a pack the hunting grounds must be extensive. Mr Thompson Seton says that in Canada the wolf has a permanent home-district and a range of about fifty miles[2]. Many generations must have elapsed before the deviation of our forebears from anthropoid habits resulted in the formation of so many packs as to necessitate the practical delimitation of hunting grounds. Then the aggressiveness of the pack turned upon strangers of its own species; the first wars arose, and perhaps cannibalism on the part of the victors. It is certain that, in N. America, wolves kill and eat foxes, dogs, coyotes;

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[1] *Wild Beasts of India*, 275, 6. Cf. Casserly, *Life in an Indian Outpost*, 94–5. Brehm says, in *Thierleben*, that in Russia wolves attack and kill the bear.

[2] *Life Hist. of N. Animals*, 754.

and it is generally believed that wolves will eat a disabled companion; though, according to Mr W. H. Hudson, a wolf will only eat another when it has killed that other, and then only as the carrying out of the instinct to eat whatever it has killed[1].

(6) A pack must have a leader, and must devotedly follow him as long as he is manifestly the best of the pack; and every individual must be subservient to the pack, as long as it works together.

(7) The members of the pack must be full of emulation; in order that, when the present leader fails, others may be ready to take his place.

(8) For the internal cohesion of the pack, there must be the equivalent of a recognised table of precedence amongst its members; and this is reconciled with the spirit of emulation, by fighting until each knows his place, followed by complete submission on the part of the inferior. Mr Th. Rooseveldt says of a pack of dogs employed in bear-hunting, "at feeding-time each took whatever his strength permitted, and each paid abject deference to whichever animal was his known superior in prowess[2]." Mr W. H. Hudson writes of dogs on cattle-breeding establishments on the pampas, that he presumes "they are very much like feral dogs and wolves in their habits. Their quarrels are incessant; but when a fight begins the head of the pack, as a rule, rushes to the spot," and tries to part the combatants—not always successfully. "But from the foremost in strength and power down to the weakest there is a gradation of authority; each one knows just how far he can go, which companion he can bully when in a bad temper or wishes to assert himself, and to which he must humbly yield in his turn[3]." The situation reminds one of a houseful of schoolboys, and of how ontogeny repeats phylogeny. Where political control is very feeble, as in mining camps or backwoods settlements, civilised men revert to the same conditions. Fifty years ago, "all along the frontier between Canada and the United States, everyone knew whom he could lick, and who could lick him[4]."

(9) A pack of wolves relies not merely upon running down its prey, but resorts to various stratagems to secure it: as by surrounding it; heading it off from cover; driving it over a precipice; arranging relays of pursuers, who take up the chase when the first begin to flag; setting some to lie in ambush while the rest drive the prey in their

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[1] *Nat. in La Plata*, 346.

[2] *Outdoor Pastimes of an American Hunter*, 70.

[3] *Op. cit.* 336–7.

[4] Hiram S. Maxim, *My Life*, 57.

direction. Such devices imply intelligent co-operation, some means of communicating ideas, patience and self-control in the interests of the pack. Failure to co-operate effectually is said to be punished with death. Primitive man, beginning with more brains than a wolf, may be supposed soon to have learnt such arts and to have improved upon them.

(10) When prey has been killed by a pack of wolves, there follows a greedy struggle over the carcase, each trying to get as big a meal as possible. Mr Th. Rooseveldt writes of dogs used in hunting the cougar (puma): "The relations of the pack amongst themselves (when feeding) were those of wild beast selfishness....They would all unite in the chase and the fierce struggle which usually closed it. But the instant the quarry was killed, each dog resumed his normal attitude of greedy anger or greedy fear toward the others[1]." As this was a scratch pack of hounds, however, we cannot perhaps infer that a naturally formed pack of wolves is equally discordant, or that the human pack was ever normally like that. Galton, indeed, says: "Many savages are so unamiable and morose as to have hardly any object in associating together, besides that of mutual support[2]." But this is not true of all savages; and, at any rate, the steadier food-supply obtained by our race since the adoption of pastoral or agricultural economy, with other circumstances, has greatly modified the greedy and morose attitude in many men and disguised it in others; though it reappears under conditions of extreme social dislocation. In the original pack it may have subserved the important utility of eliminating the weak, and of raising the average strength and ferocity. But some custom must have been established for feeding the women and children. No doubt when fruits were obtainable, the women and children largely subsisted upon them. But the strong instinct of parental care in primates, the long youth of children, and the greater relative inferiority of females to males (common to anthropoids and savages) than is found amongst dogs and wolves, must have made the human pack from the first differ in many ways from a pack of wolves.

So much, then, as to the traits of character established in primitive man by his having resorted to co-operative hunting: they plainly persist in ourselves.

On our intelligence life in the hunting pack had just as revolutionary an influence. The whole art of hunting had to be learned from its rudiments by this enterprising family. With them there was no inherited instinct or disposition, and no tradition or instruction, as there is with the true carnivores: they depended solely on observation, memory, inference. With poor olfactory sense (as usual in apes) prey must be followed, inconvenient enemies outwitted, by acquiring a knowledge of their footprints and other visible signs of neighbourhood, and by discrimination of all the noises they make. The habits and manners of prey and of enemies, their favourite lairs, feeding grounds and watering places, their paths through forest, marsh, thicket and high grass, must all be learnt: so must their speed, endurance, means and methods of attack and defence. The whole country within the range of the pack must be known, its resources and its difficulties; and whenever new territory was entered, new lessons in all these matters had to be learned. This must have entailed a rapid natural selection of brains. Only a rapidly developing, plastic brain could have been capable of the requisite accommodation of behaviour in such conditions: a mechanism was required by which more and more new lines of specialised reaction were related to numerous newly observed and discriminated facts.

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[1] *Op. cit.* 6–7.

[2] *Op. cit.* 78.

The very crudest weapons may be handled with variable dexterity; the best handling must be discovered and practised; and this had a high selective value for the hands as well as for the brain. Probably crude weapons were very early used; for some monkeys (and baboons generally) throw sticks or stones, or roll stones down upon an enemy. In Borneo, Wallace came upon a female orang who, "as soon as she saw us, began breaking off branches and the great spiny fruits [of the durian] with every appearance of rage, causing such a shower of missiles as effectually kept us from approaching too near the tree. This habit of throwing down branches has been doubted; but I have, as here narrated, observed it myself on three separate occasions[1]." The importance of the observation consists in its proving the existence in an anthropoid of the impulse to use missiles under the occasional stress of anger; so that it might be expected rapidly to develop under the constant pressure of hunger. The use of clubs and stones induced the discrimination of the best materials for such weapons, and where they could be found; and, in process of time, brought in a rough shaping of them, the better to serve their purposes.

Thus the primitive human, or prehuman mind, was active in many new directions; and depending for its skill, not upon instinct or imitation, but upon observation and memory and inference, it was necessary for it to arrange ideas in a definite order before acting upon them, as in making weapons or planning a hunt; and the contrast between growing memory and present experience, practical ideas and the actions realising them that had been suspended until the right moment came, furthered the differentiation of self-consciousness from the world.

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[1] *Malay Archipelago*, 43.

If it be asked—how much of all this development attributed to the hunting pack might have been brought about just as well by the formation of a defensive herd, such as we see in cattle and horses?—a definite answer can be given. The herd is, of course, marked by (2) gregariousness, (3) contagious sympathy and sometimes effective sympathy in common defence, (6) recognition of leaders (all herds that travel have leaders), (7) emulation, (8) precedence; but not by (1) interest in the chase and in killing, nor (4) aggressiveness, nor (9) strategy in attack, nor (10) greed; and herd-life affords no conditions for the development of intelligence and dexterity, nor for any of the physical characters that distinguish man. Herd-life does not involve the great and decisive change which is implied in the evolution of human nature. We must conceive, then, of the primitive human mind as a sort of chimpanzee mind adapted to the wolfish conditions of the hunting pack. Wolves themselves have undergone no great development, compared (say) with cats, for want of hands and other physical advantages which we had to begin with. If some species of baboon had taken to the hunting life, there might have been very interesting results.

5. THE WOLF-TYPE OF MAN ESTABLISHED BY NATURAL SELECTION.

The differentiation of the human from the anthropoid stock must have begun a long time ago; as to when it began there is no direct evidence; and even if fossil remains of the earlier stages of our evolution had been discovered, we could only judge from the strata in which they occurred what must have been their relative antiquity. When it comes to reducing the chronology of past ages to figures, geologists either decline to make any estimate, or the results of their calculations may differ as 1 to 10. Since my own studies give me no claim to an opinion on such matters, whilst it is helpful to have clear ideas, however tentative, I shall adopt the views of Dr Arthur Keith in his work on *The Antiquity of Man*, based (as he says) on estimates published by Professor Sollas. On turning to p. 509 of that work, a genealogical tree will be found, showing the probable lines of descent of the higher primates. The separation of the human from the great anthropoid stock is represented as having happened at about the last third of the

Oligocene period—say 2,000,000 years ago: which is (I understand) a decidedly moderate allowance. Pithecanthropus (of Java) branched off as a distinct genus about the middle of the Miocene. Neanderthal man (*Homo Neanderthalensis*) and Piltdown man (*Eoanthropus Dawsoni*) separated as distinct species (or genera) from the stock of modern man (absurdly named *Homo sapiens*) early in the Pliocene, and became extinct respectively (say) 20,000 and 300,000 years ago. The races of modern man began to differentiate near the end of the Pliocene (say) 500,000 years from the present time.

The skull capacity of the great anthropoids averages 500 c.c.; that of Pithecanthropus is estimated at 900 c.c.; the Australian native average is 1200 c.c.; Eoanthropus, according to Dr Keith, rises to 1400[1]; a Neanderthal skull has been measured at 1600 c.c.; the modern English average is under 1500 c.c. Dr Keith thinks it probable that men with a skull capacity of 1000 c.c. had come into existence at the commencement of the Pliocene.

As to culture, the Neolithic period extends in Western Europe from about 2,000 to 10,000 B.C.: and to that age is usually attributed the introduction of agriculture, pottery, weaving, permanent constructed dwellings, and monuments requiring collective labour. In other parts of the world, *e.g.* in the Eastern Mediterranean region, such culture is probably older, but still comparatively recent. What is known as the Palaeolithic stage of culture seems to have begun early in the second quarter of the Pleistocene period, giving us a retrospect of (say) 300,000 years. In Pliocene deposits have further been discovered numerous "eoliths": stones so roughly chipped that, whilst many archaeologists accept them as of human workmanship, some experts dispute their claim to be considered artefacts. But even in the Miocene rudely flaked flints have been found, whose claim to be artefacts is supported by their being associated with flints that "have been subjected to the action of fire[2]." Of course, there must be eoliths; the only question is whether we have yet unearthed any of them. Our forefathers cannot have begun by shaping stones to a definite figure and special purpose. Beginning with stones taken up as they lay, they discovered that a broken stone with a sharp edge inflicted a worse wound than a whole one; then broke stones to obtain this advantage; used sharp fragments to weight clubs; and very slowly advanced to the manufacture of recognisable axes and spear-heads, meanwhile discovering other uses for flaked stones; and it seems to have needed at least 1,700,000 years to arrive at the poorest of known palaeoliths. This strikingly agrees with the law, often stated, that the progress of culture is, by virtue of tradition, cumulative, and flows, as a stone falls, with accelerating velocity: in spite of the ebb, to which from age to age we see it to be liable. At any one time, moreover, the art of stone-working was, probably, even in adjacent tribes, at different stages of advancement; but it has been only recently that such contrasts could occur as Herodotus[1] describes among the hosts of Xerxes: when, beside the well-accoutred Persians and Medes, marched Libyans and Mysians armed with wooden javelins hardened in the fire, and Ethiopians with stone-tipped arrows and spears headed with the sharpened horns of antelopes.

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[1] Dr Smith Woodward's reconstruction gives the skull of Eoanthropus a capacity of 1300 c.c.

[2] Avebury, *Prehistoric Times*, 7th ed., 435.

The moral of all this is that there was abundant time before the rise of Neolithic culture (which may be called the beginning of civilization) for the complete adaptation of mankind everywhere, by natural selection, to the life of hunters; and that, since then, there has not been time for the biological adaptation of any race to the civilized state. We shall see that natural selection has probably had some civilizing influence; but any approach to complete adaptation has been impossible, not only for want of time, but also because of rapid changes in the structure of civilization, the social protection of some eccentrics, the persistence of the hunting life as a second resource or as a pastime, and by the frequent recurrence of warfare—that is to say, man-hunting. To civilization we are, for the most part, merely accommodated by experience, education, tradition and social pressure. A few people seem to be adapted to civilized life, and others to the slavish life; but all inherit, more or less manifestly, the nature of the hunter and warrior. This is a necessary basis of general and social psychology; and perhaps tribal or national characters (so far as distinguishable) may be understood by assigning the conditions under which they have, in various directions, been modified from this type.

To avoid the appearance of overlooking an obvious objection, I may add that the life of the hunter does not imply an exclusively carnivorous diet, but merely that hunting is the activity upon which his faculties are bent and upon which his livelihood chiefly depends. It is most unlikely that a cousin of the frugivorous anthropoids should entirely give up his ancestral food, immediately, or perhaps at any

[1] B. VII. cc. 69, 71, 74.

time. Even the diet of the wolf, in N.E. Canada, includes "much fruit, especially the uva-ursi"; and the coyote there also eats berries[1]; so does the jackal in India. Savage women everywhere subsist largely on roots and fruits. Dr Keith says the teeth and jaws of the Neanderthal species were adapted to a coarse vegetable diet[2]. Yet the Neanderthal burials at La Ferrasie, La Chapelle aux Saints, Jersey and Krapina, with their implements and animal remains, leave no doubt that the species hunted the biggest game. At Krapina, besides mammoth and rhinoceros, "the cave-bear occurred abundantly, it was evidently a favourite article of diet....Some of the human bones were charred, and some had been apparently split open; on that slender basis the Krapina men have been suspected of cannibalism[3]." As, no doubt, they regarded bears as bear-shaped men, they may have had as little scruple in eating man-shaped men.

6. SOME FURTHER CONSEQUENCES OF THE HUNTING LIFE.

Between the remote age when our hypothetical ancestor became a hunter and the time to which probably belong the remains of the oldest known men, there lies a gap of (say) 1,500,000 years, concerning which we have not only no direct evidence but not even any parallel in the world by means of which to apply the comparative method. Just at the beginning, the parallel of the wolf-pack sheds some light upon our path; but the light soon grows faint; for the primitive human, from the first more intelligent than wolves, and inheriting qualities of character which the new life greatly modified but could not extirpate, must under pressure of selection have become, after not many ages, an animal unlike any other. Just at the end, again, something concerning those who lived many thousand years before the beginning of history may be inferred from the parallel of existing savage customs; from their rock-dwellings, drawings, tools, weapons, hearths, something about their way of life; from evidence of their burial-customs, something of their beliefs. But what can be said of our ancestors during all those years that intervene between the beginning and the end?

Having been a hunter at the first and at the last, we may reasonably suppose that he had been so all the time. But, with our present knowledge, our chief guide as to other matters seems to be the fact that the most backward of existing savages possess powers of body and mind, and forms and products of culture, which must have been acquired gradually through a long course of development from no greater origins than are traceable in apes and wolves. As the use of good stone weapons by living savages and the occurrence of stone weapons in deposits of various age in the Pleistocene justify us in assuming that there must have been eoliths of even cruder workmanship at remoter dates, so the possession by savages of extensive languages, intricate customs, luxuriant myths, considerable reasoning powers and even humane sentiments, compel us to imagine such possessions as belonging to our prehistoric ancestors, in simpler and simpler forms, as we go back age by age toward the beginning. A tentative reconstruction of the lost series of events may sometimes be supported by what has been observed of the individual development of our children.

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[1] E. Thompson Seton, *The Arctic Prairies*, 304 and 352.

[2] *Op. cit.* 151, 239, 476.

[3] A Keith, *op. cit.* cc. vi. and vii.

(*a*) For example, the constructive impulse, slightly shown by anthropoids that make beds and shelters in the trees, was called into activity in man especially in the making of weapons and tools, and became an absorbing passion; so that a savage (often accused of being incapable of prolonged attention!) will sit for days working at a spear or an axe. Many children from about the sixth year come under the same fascination. This is a necessary preparation for all the achievements of civilized life.

(*b*) As to language—in the most general sense, as the communication of emotions and ideas by vocal sounds—the rudiments of it are widespread in animal life. A sort of dog-language is recognised, and monkeys seem to have a still greater 'vocabulary.' Hence, a number of emotional vocal expressions was probably in use among the primitive human stock. And the new hunting life was favourable to the development of communicative signs; for it depended on co-operation, which is wanting in ape-life, and in the lower extant savages hardly exists, except in hunting, war, and magical or religious rites. Hunting, moreover, is especially encouraging to onomatopoeic expression in imitating the noises of animals. It was still more favourable, perhaps, to the growth of gesture-language in imitating the behaviour of animals and the actions involved in circumventing and attacking them. Increasing powers of communication were extremely useful, and the pack must have tried to develop them. Without the endeavour to communicate, there could never have been a language better than the ape's; nor could there have been the endeavour without the need. That gesture alone was very helpful may be assumed; and it must have assisted in fixing the earliest vocal signs for things and actions, and probably determined the earliest syntax; but when, in hunting, members of the pack were hidden from one another, or when their hands were occupied, gesture was not available, and communication depended on the voice. The speech of children similarly emerges from emotional noises and impulsive babbling, assisted by gesture.

Passing to later ages, we cannot expect to learn much about the speech of prehistoric men, whom we know only by a few bones. As to the Java skull, Dr Keith observes that "the region of the brain which subserves the essentially human gift of speech, was not ape-like in Pithecanthropus. The parts for speech are there; they are small, but clearly foreshadow the arrangement of convolutions seen in modern man." On the other hand, "the higher association areas...had not reached a human level[1]." The jaw of this skull not having been found, nothing can be said of its fitness for carrying out the process of articulation. As to Eoanthropus, "if our present conception of the orbital part of the third frontal convolution is well founded, namely, that it takes part in the mechanism of speech, then we have grounds for believing that the Piltdown man had reached that point of brain-development when speech had become a possibility.. When one looks at the lower jaw, however, and the projecting canine teeth, one hesitates to allow him more than a potential ability[2]." The jaw had not undergone the characteristic changes which in modern man give freedom to the tongue in the articulation of words[3]. But Dr Keith "cannot detect any feature in the frontal, parietal or occipital areas which clearly separate this brain-cast from modern ones[4]." Eoanthropus, therefore, must have had a good deal to say and, being a social animal, must have felt the need of expression; and, though he was not a direct ancestor of ours, it can hardly be doubted that at some period the jaws of our own ancestors were no better adapted than his to articulate speech. May we not infer that articulate speech, meeting a need of the stock, arose very gradually, and was slowly differentiated from some less definite and structural connection of expressive and onomatopoeic vocables, such as we have seen may naturally have arisen amongst the earliest hunters?

(c) All savages live by custom; gregarious animals have their customs; and in the primitive hunting pack customs must have been early established as 'conditions of gregariousness.' M. Salomon Reinach, indeed, thinks that the anthropoid probably became human as the result of inventing taboos, especially in sexual relations; there was economy of nervous energy in the direction of the senses, and consequent enrichment of the intellect[1]. This hypothesis does not carry us far, perhaps, into the particulars of human character; but it contains this truth, that without the growth of customs there could have been no progress for human nature; and it certainly points to the probability that some custom was early established with regard to marriage. In Professor Westermarck's opinion our species was originally monogamous[2]. Supposing this to have been the custom, as it is amongst the anthropoids, could it have persisted after the formation of the hunting pack? According to Mr Thompson Seton, wolves pair "probably for life[3]"; but this is disputed; and so it is whether or no the male of a seasonal pair takes part in caring for the puppies. Of the primitive human stock one may say that whilst, on the one hand, the association of many males and females in the same pack may have tended to break up the family, on the other hand, the long youth of the children and the parental care generally characteristic of primates would have tended to preserve it; that the practice of pairing requires the largest number of males, and lessens quarrelling, and is therefore favourable to the strength of the pack; and that any custom may have been established that was most favourable to the species in its new life. The least probable of all conditions is promiscuity.

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[1] *Antiquity of Man*, 268.

[2] A. Keith, *op. cit.* 408.

[3] *Op. cit.* 452.

[4] *Op. cit.* 414.

(*d*) The claim to property is instinctive in many animals—claim to a certain territory, or to a nest, or lair, or mate. Each early human pack probably claimed a certain hunting-range; and each family its lair, which it guarded, as our domestic dog guards the house. As weapons or other implements, charms, or ornaments came into use, the attitude toward the territory or lair will have been extended to include them; for it seems to be instinctive even in lower primates. "In the Zoological Gardens," says Darwin, "a monkey, which had weak teeth, used to break open nuts with a stone; and I was assured by the keepers that, after using the stone, he hid it in the straw, and would not let any other monkey touch it. Here, then, we have the idea of property[4]." Among the half-wolf train-dogs of Canada, the claims of one to property seem to be recognised by others; for a dog will defend its *cache* of food against another that ordinarily it fears;

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[1] *Cultes, Mythes et Religion*, III. 430.

[2] *Primitive Marriage*, c. iii.

[3] *Life Histories of Northern Animals*, 757.

[4] *Descent of Man*, c. iii.

and "the bigger dog rarely presses the point[1]." The utility of keeping the peace within the tribe, no doubt, led to the growth of customs concerning property, and to their protection by the social sanction, and later by the taboo[2]. For taboo cannot be the origin of respect for property or for any custom: it implies a custom already existing, which it protects by the growth of a belief that is effective even when there are no witnesses. The same utility of order must have established customs of dividing the kill of the pack: later also protected by taboo, as we still see in many savage tribes.

The attitude towards property is very variable amongst the tribes now known to us. Still, considering how early and strongly it is manifested by children, we may infer with some plausibility its antiquity in the race. The urgent desire of property, and tenacity in holding it, displayed by many individuals, though not an amiable, has been a highly useful trait, to which is due that accumulation of capital that has made possible the whole of our material and much of our spiritual civilization. Amongst barbarians it may be a necessary condition of social order. Had not wealth been highly prized amongst our own ancestors, it is hard to see how revenge could ever have been appeased by the wergeld. The payment, indeed, was not the whole transaction, but implied an acknowledgment of guilt and of the obligation to make amends; but these things would not have mollified an enemy nurtured in the tradition of the blood-feud, if silver had not been dear to him. It is still accepted as compensation for injuries that seem difficult to measure by the ounce. Wealth gives rank, and gratifies not only the greed but also the emulative spirit of the pack. Acquisitiveness is an essential trait of aristocracy. Homespun prudence belongs, in our ancestry, to a more recent stratum of motives: we see it as a blind instinct in squirrels and beavers, a quasi-instinctive propensity in dogs and wolves (who hide food that they cannot immediately devour); but it is not known in any anthropoid, and is acquired at some stage by some human races—not by all; for it is not found in many extant savages. It is not, however, merely a function of intelligence, or else the Irish would be as prudent as the Scotch.

(*e*) Most of the amusements and occupations of mankind depend for their zest upon the spirit of hunting and fighting, which they gratify and relieve, either directly or in a conventionalised and symbolical way, and at the same time keep alive. Sports and games involve the pursuit of some end by skill and strategy, often the seizing upon some sort of prey, or slaying outright, and give full scope to emulation. Emulation is a motive in the race for wealth, in every honourable career, even in addiction to science and learning: though here the stress is upon an instinct older than the pack—curiosity, derived chiefly from the primates. That children at first play alone, later play together, and then 'make up sides,' repeats the change from the comparatively solitary life of anthropoids to the social life and combined activities of the hunting pack.

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[1] E. Thompson Seton, *Life Histories of Northern Animals*. 769.

[2] E. Westermarck, *Origin and Development of Moral Ideas*, II. 52.

(*f*) We are left to speculate about the early growth of magnanimity, friendliness, compassion, general benevolence, and other virtues. They cannot be explained merely by the hunting life, which so easily accounts for greed, cruelty, pride and every sort of aggressiveness. Robert Hartmann writes: "It is well known that both rude and civilised peoples are capable of showing unspeakable and, as it is erroneously termed, inhuman cruelty towards each other. These acts of cruelty, murder and rapine are often the result of the inexorable logic of national characteristics and, unhappily, are truly human, since nothing like them can be traced in the animal world. It would, for instance, be a grave mistake to compare a tiger with a bloodthirsty executioner of the reign of terror, since the former only satisfies his natural appetite in preying on other animals The atrocities of the trials for witchcraft, the indiscriminate slaughter committed by the Negroes on the coast of Guinea, the sacrifice of human victims by the Khonds, the dismemberment of living men by the Battas, find no parallel in the habits of animals in their savage state. And such a comparison is, above all, impossible in the case of anthropoids, which display no hostility toward men or other animals unless they are first attacked. In this respect the anthropoid ape stands on a higher plane than many men[1]." Are we, then, to explain the more amiable side of human nature, partly at least, by derivation from the frugivorous primates, extensively modified by our wolfish adaptation, but surviving as latent character?

Several further considerations may be offered to account for the growth of what we call humanity. (i) The long nonage of human children is favourable to the attachments of family life, and such attachments may under certain conditions be capable of extension beyond the family; but I cannot trace the whole flood of altruistic regard to the sole source of maternal or parental love. (ii) Friendliness and the disposition to mutual aid are so useful to a hunting pack that is not merely seasonal but permanent (as I take ours to have been), both to individuals and to the pack as a whole, within certain limits (as that the wounded, sick, or aged must not amount to an encumbrance), that we may suppose natural selection to have favoured the growth of effective sympathy, not merely in mutual defence, but so far as it is actually found at present in backward tribes. It nowhere seems to be excessive; and its manifestation in some civilized races seems to depend, not upon a positive increase of benevolence, but (iii) upon the breaking down of conditions that elsewhere oppose and inhibit it. Thus the generosity, mercy, magnanimity, that constitute the chivalrous ideal, depend (I believe) upon the attainment by a class of such undisputed superiority that there is no occasion for jealousy or rivalry in relation to other classes; for should the superiority be disputed, these virtues quickly disappear. Similarly, what have been called the 'slavish virtues' of charity, humility, long-suffering may arise amongst those who are free from rivalry, because they have no hope of aggrandisement in wealth or honour, and who have indeed suffered long. With the interfusion of classes, their virtues naturally interfuse; for they have a common root, and are active, provided that circumstances do not inhibit them

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[1] *Anthropoid Apes*, 294–5.

(iv) After the introduction of agriculture, the stress of natural selection was in certain directions altered; and although there has not been time for it to be very effective, there must have been some result. At first, indeed, most agricultural work was probably done by women; but, in its progress, it fell extensively into the hands of men; and then advantage accrued to those tribes who were capable of steady industry and prudence. The new employment decreased aggression on the principle that "had Alexander been holding the plough, he could not have run his friend Clitus through with a spear." The sick and aged were now less an encumbrance than they had been to hunters. Those who could not endure a settled life, wandered away in their old pursuits. The more aggressive clans slaughtered one another in the vendetta. Social pressure and hanging eliminated many of the more idle, improvident, dishonest and unruly, whose instincts resisted 'accommodation.' The more neighbourly and co-operative tended to predominate. As civilization intensifies, the numerous ways of getting a livelihood, which (as we have seen) derive their motive force from the spirit of the pack, gratify that spirit under so many disguises and with so little direct personal collision, as to be compatible with a great deal of friendliness and benevolence; and co-operation, direct or indirect, steadily increases.

(v) Reflection upon our lot has done much to ameliorate it: the conditions of gregariousness have been expounded by the more comprehensive minds; by some prophets, poets, philosophers. Increasing capacity of forming ideas of remote ends and of coordinating many activities in their pursuit, implies the inhibition of many aggressive impulses.

(*g*) The first wars probably were waged for hunting grounds; and this was a revival for the carnivorous human pack of a state of affairs that may have existed amongst their ancestors at a much earlier date; for battles for a feeding-ground have been witnessed between troops of the lower primates. A battle between two bands of langur (*Semnopithecus entellus*) is described in the *Royal Natural History,* I. 72–3. In the *Descent of Man,* c. iii., Darwin relates, after Brehm, that "in Abyssinia, when baboons of one species (*C. gelada*) descend in troops from the mountains to plunder the fields, they sometimes encounter troops of another species (*C. hamadryas*) and then a fight ensues The Geladas roll down great stones, which the Hamadryas try to avoid, and then both species, making a great uproar, rush furiously against each other." Wars have led through conquests to caste and to slavery, with their distinctive virtues and vices; to the politeness of armed men (generally) and their pride; to the mutual devotion of comrades and to the fanatical worship of power. They strengthened the internal sympathies of the pack or tribe, and its external antipathies; and extended the range and influence of the more virile and capable races.

7. INFLUENCE OF THE IMAGINARY ENVIRONMENT.

The necessity of learning the whole art of hunting from its rudiments, without the help of instinct or tradition, by sheer observation, memory and inference, put extraordinary stress upon the brain. At first by knowledge, strategy, co-operation and persistence of will, later by devising weapons and snares, evolving language and discovering the ways of making and utilising fire, man found means of entirely changing the conditions of his life; but this would have been impossible without a great development of his brain; and, accordingly, it appears that Eoanthropus, at the beginning of the Pleistocene, had a skull with nearly three times the cubic capacity of the anthropoids. With the growth of the brain came a continually increasing fecundity of ideas. "Piltdown man saw, heard, felt, thought, and dreamt much as we do[1]." The use of ideas is to foresee events and prepare for them beforehand:

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[1] A. Keith, *op. cit.* 429.

the great advantage of distance-senses over contact-senses, is to give an animal time to adapt its actions to deferred events; and ideas give this power in a vastly higher degree. So far the utility of brains and ideas seems obvious. But in order that ideas may be useful in this way, they must (one would suppose) represent and anticipate the actual course of events. If they falsely indicate the order of nature, or even beings and actions that do not exist at all, ideas may seem to be worse than useless.

Now, when we turn to the lowest existing savages, they are found to possess a considerable fecundity of ideas; constituting, on the one hand, a good stock of common sense, or knowledge of the properties and activities of the things and animals around them, and of how to deal with them, which enables them to carry on the affairs of a life much more complex and continuous than any animal's: but including, on the other hand, a strange collection of beliefs about magic and animism, which entirely misrepresent the course of nature and the effective population of the world. These latter beliefs, or imaginative delusions, hamper them in so many ways, waste so much time, lead them sometimes into such dark and cruel practices, that one may be excused for wondering whether their bigger brains can have been, on the whole, of any biological advantage to them in comparison with the anthropoids. The anthropoids live by common sense. So do savages, and they have much more of it; but the anthropoids seem not to be troubled by magic and animism. We must suppose that the common sense of primitive man increased age by age, as he became more and more perfectly adapted to the hunting life, and that at some stage his imagination began to falsify the relations of things and the powers of nature. I have tried to show that imagination-beliefs depend chiefly upon (*a*) the influence of desire and fear, (*b*) hasty generalization, and (*c*) the seduction of reasoning by analogy. At what stage imaginations, thus dissociated from reality, began to have an important part in human life, it is impossible to say; but it cannot be less than half a million years ago, if (as Dr Keith says) Eoanthropus, 400,000 years ago, "thought and dreamt much as we do." Why did not such illusions hinder our development? Or did they promote it?

The first consideration is, that biological adaptation is nearly always a compromise: if any organ or faculty is useful on the whole, in spite of some disutility, its increase favours the survival of those in whom it increases; and this is true of the brain and its thinking. The second is, that nearly all the magical and animistic beliefs and practices that are socially destructive, belong to an advanced stage of human life, long after our differentiation has been established, and when some progress has been made in arts and institutions. Savages of the lowest culture have few beliefs that can be called positively injurious. Talismans and spells, not by themselves relied upon, but only adscititious to common-sense actions, give confidence without weakening endeavour. To curse, or to 'point the bone,' does not create but merely expresses a malevolent purpose; and, although sometimes fatal by suggestion, is on the whole better than to assassinate. Taboos do more good by protecting person and property than they do harm by restricting the use of foods. Belief in imaginary evils waiting upon secret sins exerts, whilst supported by social unanimity, a control upon all kinds of behaviour: it is the beginning of the 'religious sanction,' and one sort of conscience. The dread of spirits that prowl at night keeps people in the family cave or by the camp-fire; and that is the best place for them. Many rites and observances are sanitary. Totemism rarely does much harm, and may once have been the necessary symbolism of tribal unity. Totemic and magical dances give excellent physical training, promote the spirit of co-operation, are a sort of drill; and (like all art), whilst indulging, they also restrain imagination by imposing upon it definite forms. For a long time there was no special profession of wizard or priest, with whose appearance most of the evil of magic and animism originates; though probably even they do more good than harm by their courage and sagacity, by discovering drugs and poisons, by laying ghosts, and by their primitive studies in medicine and psychology. When hunting in pack is no longer man's chief task, the leader of a clan cannot be plainly indicated; and then the reputation of old men for magic enables them to maintain tradition and custom: they become headmen or chiefs. The necessary development of imagination along with common-sense, therefore, assisted early culture. Excessively imaginative and superstitious tribes may sometimes have been eliminated. If one quality more than any other is likely to be favoured by natural selection, it is common sense. But imaginations literally untrue have had their part in promoting the later progress of the human race; co-operating with agriculture and trade, magic religion and the fine arts have, by supporting government and civil order, helped in accommodating us to our present condition, such as it is.

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ON THE FEELINGS AND THEIR NEURAL CORRELATE, WITH AN EXAMINATION OF THE NATURE OF PAIN.

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I. *Logical argument.*
II. *The nature of pain.*
III. *The neural correlate of the feelings.*
IV. *Summary.*

I.

THE purpose of the present paper is to point out a logical fallacy expressed or implied in many psychological systems and to examine its bearing on the question of the neural correlate of Feeling.

If we have given the premises

All A is B,

and All C is A,

then the logical conclusion about C is

All C is B.

If we find the conclusion 'All C is D,' and it is evident that 'No D is B,' then a fallacy has been committed. Let us therefore examine the premises, beginning with the major.

THE MAJOR PREMISE.

The major premise is a proposition respecting the relation between body or brain, and mind or soul or consciousness, etc., and any statement giving expression to any of the many varied forms of parallelism concerns our enquiry. The doctrine, or hypothesis, of psychophysical parallelism is of a rather protean nature and endless shades of variations are possible. The usual conception of parallelism may be applied to a monistic doctrine of identity equally well as to certain forms of definite dualism. All that is necessary for our purpose is the assumption that every elementary process of consciousness corresponds or correlates to certain neurone-processes or cerebral excitations. Whether this relation or correspondence is conceived as identity of the neurone-process with consciousness on the one hand, or interaction between the brain and the soul on the other, is immaterial. We may then express this postulate, which forms the major premise, as follows:

All Elementary Processes of Consciousness correspond to Definite Neurone-processes (run parallel to them—are identical with them—are produced by them—depend upon them—are causally connected with them, etc., etc.).

Let us next cite a few examples:

(1) Alfred Lehmann: "Energy occurs in several different forms, of which some arise very frequently, but others only under quite definite conditions. We can look upon the psychic as such a form of energy which has, up to the present, been shown to exist only in the central nervous system. For if the psychic is conceived as a form of energy, that arises in the nervous system by transformation of other forms of energy, the psychological laws can then be only laws of nervous function[1]."

(2) Hermann Ebbinghaus: "Soul and nervous system are not something really separate, something opposed one to the other. They are one and the same real union, but in different and divergent modes of manifestation. Soul is this copious (reichhaltig) union, as it shows and manifests itself to its own members, to the part-realities belonging to it. Brain is the same union as it reveals itself to other analogically built unions, when—to speak humanly—it is seen or touched by these[2]."

"The members of the one series do not produce those of the other series, nor do they interfere with them in any way. The one series, with respect to the real happening (dem realen Geschehen nach), is rather absolutely identical with the other series, and the twoness (duality), which is indeed also present, depends solely upon the richness of the other existence in the world[3]."

"Our theory of the relations of the psychical to the material asserts then: whenever there happen in a soul thought, desires, etc., and whenever there occur simultaneously what we call in the inadequate views and expressions of our soul the being seen or being touched, then those thoughts and desires do not simply exist, but *are viewed, or may be viewed, at the same time* as definite material and specially nervous processes[1]."

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[1] *Grundzuge der Psychologie*, Leipzig, 1912, p. 28.

[2] *Grundzuge der Psychologie*, 1te Aufl., Leipzig, 1902, Bd 1, p. 42.

[3] *Loc. cit.* p. 43.

(3) William James: "The fact that the brain is the one immediate bodily condition of the mental operations is indeed so universally admitted nowadays that I need spend no more time in illustrating it, but simply postulate it and pass on[2]."

"The consciousness, which is itself an integral thing not made up of parts, 'corresponds' to the entire activity of the brain, whatever that may be, at the moment[3]."

"...We must return to its consideration again, and ask ourselves whether, after all, the ascertainment of a blank unmediated correspondence, term for term, of the succession of states of consciousness with the succession of total brain processes, be not the simplest psychophysic formula, and the last word of a psychology, which contents itself with verifiable laws, and seeks only to be clear, and to avoid unsafe hypotheses[4]."

(4) H. Höffding: "It is perhaps subject to no doubt, that even the very highest functions of consciousness have their corresponding brain functions[5]."

"The parallelism as well as the proportionality between activity of consciousness and brain-activity point to a fundamental identity[6]."

"The physiological connection and the interaction between the exceedingly numerous centres of the brain give besides a sufficient basis for the supposition, that not only the psychical elements but also the compounds of these possess their physical expression[7]."

(5) Oswald Külpe: "That this (the dependence of the experiences upon the corporeal individual) exists at all, has up to the present only been denied by a certain class of metaphysicians.......According to this the bodily processes which are in direct functional relationship to the experiences, are in man to be found entirely in the brain, probably in the cortex......For this reason one generally speaks at present of a parallelism between the psychical and brain processes, *i.e.* one imagines them as phenomena accompanying each other and of such nature,

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[1] *Loc. cit.* p. 46.
[2] *The Principles of Psychology*, 2 vols., London, 1901, I. 4.
[3] *Loc. cit.* I. 177.
[4] *Loc. cit.* I. 182.
[5] *Psychologie in Umrissen auf Grundlage der Erfahrung*, Uebersetzt von Bendixen, Leipzig, 1887, p. 75.
[6] *Loc. cit.* p. 80.
[7] *Loc. cit.* p. 82.

that any change on the one side manifests itself by a corresponding change on the other side[1]."

(6) Wilhelm Ostwald: "Accordingly I suggest to you to conceive consciousness as a property of a special kind of nerve-energy, viz. of that which manifests itself in the central organ......In what manner then have we to regard consciousness and nerve-energy as connected? It seems to me that this connection is to be conceived as close as possible, and I am inclined to regard consciousness as just as characteristic a mark of nerve-energy of the central organ, as, *e.g.* the spacial quality is a characteristic mark of mechanical energy, and the temporal quality one of kinetic energy......It gives me no more difficulty to think that kinetic energy causes movement than that the energy of the central nervous system causes consciousness[2]."

The selection of statements respecting the relation of the psychical to the physical has been copious and varied. They all agree, however, with the general formula given above. Let us now turn to

THE MINOR PREMISE.

This proposition is a statement concerning Feelings. There are extant three principal theories concerning these. The first is that which regards the feelings as attributes or *functions* of sensations or other cognitive processes of consciousness, analogous to quality and intensity. The other two theories agree in that they affirm that the feelings are definite states of consciousness, and not functions or attributes, but they differ in that the one regards them as sensations, whilst the other teaches that they are a definite and separate class of mental experiences.

In order to dispose of the first of these theories we may shortly recapitulate the principal reasons, generally advanced to disprove that the feelings are qualities or functions of the cognitive states. Let us, for the sake of simplicity, consider here merely such typical elements of our conscious life, as the sensations. The attributes of a sensation are:

(1) Quality,
(2) Intensity,
(3) Duration, and in most cases also
(4) Extensity.

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[1] I. *Grundriss der Psychologie*, Leipzig, 1893, p. 4.—II. *Zur Psychologie der Gefuhle*, 6th International Congress of Psychology. Geneva, 1909, p. 181 *seq.*

[2] *Vorlesungen uber Naturphilosophie*, 2te Auflage, Leipzig, 1902, pp. 393–396.

Now if any one of these attributes becomes zero the sensation does *ipso facto* no longer exist. A sensation without a quality is unthinkable. If the intensity disappears the sensation disappears with it, and a sensation without duration is an absurdity. With extensity this is not so, for there are sensations, *e.g.* those of smell, which are not generally regarded to possess extensity, and in the case of auditory sensations this is a very much disputed point. However, whenever a sensation has the attribute of extensity, *e.g.* red, it ceases to exist with the disappearance of its extensity.

With the feeling-tone this is quite different. Sometimes sensations have no feeling-tone at all, they are neutral; but frequently they possess a very pronounced feeling-tone which, however, may alter remarkably, although the sensation continues with no appreciable change in quality or intensity. If it was pleasant it may become indifferent, or even actually unpleasant; or a sensation may on one occurrence be pleasant, and a similar sensation on another occasion may be unpleasant.

Another point that may be advanced to support this view is the want of a simultaneity of sensation and feeling. Thus, *e.g.*, Titchener[1] states: "It is by no means uncommon—*e.g.* in experimental work upon the association of ideas—to find cases recorded in which a feeling precedes or lags behind or outlasts its idea." Further Ladd[2]: "...The general truth is that in the flow of the one stream of conscious life the feeling may assume either one of the three possible time-relations towards the sensations and ideas by which we classify them; they may fuse with them in the 'now' of the same conscious state, or they may lead or follow them."

This question has been investigated also experimentally by Scripture[3] who comes to the following conclusion: "In every state of mental life perceptions and feelings are met with as component parts. Both possess various constantly changing degrees of awareness, from the maximal degree of most acute attention down to the unconscious. This is the reason that now the perception, now the feeling is the predominant element, and, in fact, appears to be the only one. We may have perceptions without being conscious of the accompanying feelings, and we may with a strong feeling entirely disregard the accompanying perception. As parts of the psychical life perceptions and feelings are, however, always connected one with another; they are coordinate part phenomena of the psychical process and, independent of the degree of awareness, it is now the feeling, now the perception, now the compound of both, that is of influence on the course of perceptions."

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[1] *Lectures on the Elementary Psychology of Feeling and Attention*, New York, 1908, p. 42.

[2] *Psychology, Descriptive and Explanatory*, 1894, p. 181.

[3] "Vorstellung und Gefühl," *Wundt's Philosophische Studien*, Bd VI., 1891, pp. 536–542.

Baldwin says: "As concrete facts, pleasure and pain are always elements added to some conscious content. It is in this aspect that they are described as *tone*, the states of which they are the tone being more or less exactly discoverable[1]."

There are also certain states of consciousness, states which we may call 'laisser vivre,' probably identical with the 'dolce far niente,' where introspection, or better, retro-introspection, seems to reveal that all that we have been conscious of, was a state of a merely highly affective nature, exceedingly pleasant. The cognitive content was, so to speak, nil, or if present, transitory, fleeting, constantly changing and quite independent of the affective content of consciousness, which remained constant. It is argued that these affective states of consciousness are borne by the body-sensations. Thus Wundt[2] says: "The dominant feelings are probably mostly attached to the sensations of gentle muscle-tension, that are derived from the position and movements of the limbs, from the respiratory movements, and which are occasionally replaced by the sensations of the chemical senses." This is merely the cœnesthesis of the old physiology revived. But, although the existence of the common or body sensations cannot be denied, to assume processes of consciousness to be cognitive when no cognitive content can be detected by introspection, but only affective content, is mere dialectics. A similar view is held by Külpe[3] who says: "We can quite easily make the so-called feeling-tone disappear without destroying thereby the sensation itself. In connection with this statement we find then sensations without feeling, *i.e.* sensations that are neither pleasant nor unpleasant, and, according to my experience, we find also feelings without sensation, that is to say, feelings such as are not accompanied or borne by a definite sensation, and such in which the nervous conditions of the sensations are prevented from the exercise of their usual effects on consciousness."

We find still further evidence for our contention in those emotional dispositions known as *Moods*. Whatever the cognitive content of consciousness, the feeling-tone of the given mood fastens itself upon it, or, as Stout puts it: "When these moods are once in existence they create objects for themselves[1]." In fact we can invert Wundt's statement that the body sensations, *i.e.* the cognitive content of consciousness, are the substratum of the feelings, and say with equal truth of the moods, that the affective content of consciousness is the substratum of the cognitive content.

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[1] *Handbook of Psychology*, "Feeling and Will," 1891, p. 87.

[2] *Grundzuge der physiologischen Psychologie*, 5te Aufl. 1902, II. 348.

[3] *Loc. cit.* I. p. 233.

We may note here another difference between Sensation and Feeling. Centrally excited sensations are of another kind, or are held by some to be of the same kind as, but much weaker than, peripherally excited sensations, whilst centrally excited feelings are of the same kind as, and are frequently much stronger than, peripherally excited ones. Or putting it in other words, we may say: There are memory images of cognitive experiences but not of affective experiences. An 'ekphored' feeling is always a new state of feeling and never the memory image of a previous one.

We will next examine whether Feeling is a *function* of sensation. Külpe[2] says: "Feeling could be regarded as a function of sensation in so far as the properties of the latter determine by themselves the behaviour of feeling, that is, in so far as a parallelism between the two exists (similarly as sensation and stimulus stand in functional relation to each other), or in so far as certain combinations of sensations are regarded as factors determining feeling."

But, as he points out, this latter view is untenable, for self-observation shows us constantly, that there are sensations which are always pleasant or unpleasant to us, no matter what be their relation to other sensations. Toothache, he says, remains unpleasant even if the other sensations occurring in consciousness possess properties that are neither harmonizing with, nor antagonistic to it.

The other view, according to which feeling ought to be regarded as a function of simple sensations, Külpe considers just as one-sided, since without a doubt feeling may be connected with the relation of the sensations to each other. To show also that a combination of these two views would not do justice to our experience, Külpe[3] proceeds to examine the dependence of feeling on the different sensations, or, what is equivalent to this, the properties of the sensation: "Evidently feeling does not depend upon the *Quality* of the sensation. There are no sensation-qualities, which as such have a pleasant or unpleasant feeling-tone, or as such are accompanied by stronger or weaker, longer or shorter feelings. No more can it be maintained that every sensation-quality has its own particular feeling-tone, which as pleasantness or unpleasantness can be distinguished specifically from the feeling-tone accompanying other sensations, for we should then have twice as many feeling-qualities as there are sensations, which is against our experience."

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[1] *Manual of Psychology*, 2nd ed., London, 1901, p. 299.

[2] *Loc. cit.* I. 234.

[3] *Loc. cit.* p. 235.

For this reason it is often asserted that feeling is a function of the *Intensity* of the sensation. But although it may be true in some instances that moderately strong sensations are pleasant and strong ones unpleasant, there are so many exceptions, that this position also becomes untenable. Where sensations are very faint, as in a very slight touch of dyspepsia, the feeling-tone is generally highly unpleasant. Whilst low tones whose intensity is so strong as to excite the end-organs of touch are frequently pleasant, feeble high tones, produced by vibrating rods or tuning-forks, are generally distinctly unpleasant.

The same is the case with the *Temporal and Spatial Attributes* of sensation. Conceived as *Duration* and *Extensity*, their relation to the feelings is generally the same as that of intensity.

Since we have shown then that the Feelings are neither attributes nor a function of sensation, there remains the only other possibility, namely, their being definite processes of consciousness. We have therefore to regard the feelings as definite processes of consciousness, that is as sensation or as a separate class. Although some psychologists (Stumpf, etc.) hold that feelings are sensations the bulk of the evidence still points towards the view that they form a distinct class of mental states. As, however, a decision between these contending views is of no consequence for our argument, we shall not enter upon this question here.

The minor premise, as we have proved, is then as follows: *The Feelings are elementary states of consciousness,* and all the authors we have quoted in illustration of the major premise agree with this minor premise. We may cite:

(1) Alfred Lehmann: "In the emotional states we meet with two new elements, pleasantness and unpleasantness, which we may designate by the common name: Feeling-Elements[1]."

"As emphasized above, not only the simple sensations but also the perceptions and ideas composed of them are mostly accompanied by special states of pleasantness and unpleasantness. We distinguish these accompanying phenomena from the intellectual states mentioned as the emotional elements[1]."

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[1] *Loc. cit.* p. 176.

(2) Hermann Ebbinghaus: "Of importance is the correct (*i.e.* corresponding to our experience) comprehension of the relation of the feelings to the sensations and perceptions. A twofold error is here to be avoided. First, they do not stand so independently by the side of sensations and perceptions as the different kinds of these do, *e.g.* visual and auditory sensations......On the other hand, one must not conceive the connection (Gebundenheit) of the feelings with their accompanying sensations and perceptions as too close a one. The relation is not to be regarded as similar to that which exists, for instance, between sensations of colour and their spatial extensity, or between perception and its sensory vividness, that is to say, the feeling-tone appertaining to an intellectual content is not to be regarded as a property inalienably attached to it, which stands on the same level with its other properties, *e.g.* a given quality, intensity, duration, etc.......In order to do justice to the peculiar mixture of dependence and independence, which we find here, it is only left to us to conceive feelings as phenomena consequent upon sensations and perceptions, phenomena which are brought about by the influence of these latter on that other factor. Or perhaps better as secondary consequences of the same causes which produce the accompanying sensations and perceptions, so that there is simultaneously produced by the influence of those causes upon certain structures of the organism the intellectual effect, and by the influence upon other structures the *affective* effect belonging to it[2]."

(3) William James: "They (*i.e.* the special processes invoked to account for the emotions) are *sensational* processes, processes due to inward currents set up by physical happenings[3]."

Vol. II. p. 551, James speaks of "feelings of pleasure and pain" and in vol. I. p. 186, he defines "feelings" as a state of consciousness, or psychosis.

In a later paper[4] he is forced to admit explicitly the existence of the primary feelings: "I am willing to admit that the primary 'Gefühlston' may vary enormously in distinctness in different men...... I allow them (*i.e.* the feelings) hypothetically to exist, however, in the form of the 'subtler' emotions, and in the mere intrinsic agreeableness of particular sensations, images and thought-processes, when no obvious organic excitement is aroused."

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[1] *Loc. cit.* p. 358.

[2] *Loc. cit.* pp. 541, 542.

[3] *Loc. cit.* II. 453.

[4] "The Physical Basis of the Emotions," *Psychological Review*, 1894, I. 524

(4) H. Höffding: "The independence of the affective elements as opposed to other elements of consciousness is demonstrated by the fact that even if there should not be such a state that could be called feeling alone without cognition or conation, the feeling is nevertheless not necessarily attached to a given theoretical or practical relation. In different individuals and in the same individual at different periods pleasantness and unpleasantness are found connected with different objects. Something that at first caused unpleasantness may later cause pleasantness, or *vice versa*. A great contrast is shown at the same time between those states where thought and action so strongly occupy consciousness that the affective wave is hardly noticeable and those where the powerful emotion subdues clear thinking and prudent action[1]."

"In some experiences feeling stands out prominently as an element different from sensation proper. These experiences are those which demonstrate, that the pain caused by a stimulus requires longer time for its production than the sensation proper and that there may be sensation without corresponding feeling and *vice versa*[2]."

"It appears to be true of the feeling of pleasure as well as of the feeling of pain that in themselves they show no generic differences, but that the differences of the feelings of pleasure arise from the accompanying sensations[3]."

(5) Oswald Külpe: "Accordingly we cannot assume a simple functional relation between sensation and feeling. There remains therefore only the last of the three possible views concerning the reciprocal relation of sensation and feeling, according to which feeling has to be regarded as an independent state of consciousness. In fact, all the reasons which we advanced against the other hypotheses pointed, without exception, to this view, which we will therefore accept as the correct one, without prejudice to the constant combination in which sensation and feeling occur in consciousness[4]."

"The Feelings are elementary contents of consciousness[5]."

(6) Wilhelm Ostwald: "There is constant transition from the conscious sense-perception to the less conscious ones and at last to those where only changes or hindrances become conscious. From this results a constant transition from the sensations to the feelings[6]."

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[1] *Loc. cit.* p. 110.
[2] *Loc. cit.* p. 280.
[3] *Loc. cit.* p. 282.
[4] *Loc. cit.* I. p. 236.
[5] *Loc. cit.* II. 181 *seq.*
[6] *Loc. cit.* p. 387.

THE CONCLUSION.

From the premises enunciated above, viz.

Major: All elementary states of consciousness correspond to definite neurone-processes;

Minor: The feelings are elementary states of consciousness; there follows the

Conclusion: The feelings correspond to definite neurone-processes.

Let us, however, examine what the authors, whose writings we have cited above as in agreement with our premises, have to say respecting the basis of the Feelings.

(1) Alfred Lehmann: "Just as the sensation is determined by the quality, the intensity, the extensity and the duration of dissimilation, so the feeling-tone may be dependent upon the amount of assimilation and its relation to dissimilation, the so-called biotonus of the working neurones......If during the activity of a central neurone-group, the dissimilation is equal to the assimilation, $\frac{A}{D} = 1$, then this condition of the biotonus manifests itself psychically as pleasantness, which increases with increasing values of the dissimilation and assimilation. If, however, the dissimilation becomes greater than the assimilation, *i.e.* the biotonus decreases, $\frac{A}{D} < 1$, then this condition manifests itself psychically as unpleasantness, which increases with the decrease of the value $\frac{A}{D}$[1]."

It should follow from Lehmann's premises (p. 424 and p. 430) that the feelings depended upon definite neural processes, whilst he attributes them to the biotonus, the relation of dissimilation to assimilation of the same neurone-groups whose excitation corresponds to the sensation. It suffices for our purpose to establish this logical fallacy, and we need not therefore enter upon certain obscurities of his hypothesis, as *e.g.* that since $\frac{A}{D} = 1$ is equivalent to pleasantness which increases with simultaneously increasing values of both A and D, and unpleasantness is equivalent to $\frac{A}{D} < 1$ which increases with increasing value of D only, it is obscure what happens if both A and D increase without altering the value $\frac{A}{D} < 1$. Further what value of $\frac{A}{D}$ corresponds to a neutral feeling, or the absence of feeling? What happens if $\frac{A}{D} > 1$? for this must of necessity occur after a lengthy unpleasantness, a state, where according to Lehmann, $D > A$ all the time.

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[1] *Loc. cit.* pp. 368–9.

(2) Hermann Ebbinghaus: "The relation for the weal and woe of the organism that forms the objective basis of the feelings must somehow assume material form in order to obtain for the soul that characteristic significance, that is to say, it must manifest itself concretely in definite material processes, which in their turn are able to affect the nervous system[1]."

This statement of Ebbinghaus' reads, taken by itself, as if some organic processes are produced which in their turn stimulate a brain-centre, thus giving rise to feeling. That this is, however, not meant, that these processes are not brain-processes, is evident from the fact that Ebbinghaus cites Lehmann's theory of the ratio of assimilation to dissimilation, and Meynert's view, that feeling is an expression of the state of nutrition of the cortex, as examples of what he means by "definite material processes." In the later editions Ziehen's theory of the dischargeability of excitation as feeling-producing process is also given as a possible explanation.

(3) William James: "No special brain-centre for emotion" is the heading of a paragraph. In this James says: "Supposing the cortex to contain parts, liable to be excited by changes in each special sense-organ, in each portion of the skin, in each muscle, each joint, and each viscus, and to contain absolutely nothing else, we still have a schem capable of representing the process of the emotions. An object falls on a sense-organ, affects a cortical part, and is perceived; or else the latter, excited inwardly, gives rise to an idea of the same object. Quick as a flash, the reflex currents pass down through their preordained channels, alter the condition of muscle, skin and viscus; and these alterations, perceived, like the original object, in as many portions of the cortex, combine with it in consciousness and transform it from an object-simply-apprehended into an object-emotionally-felt. No new principles have to be invoked, nothing postulated beyond the ordinary reflex circuits, and the local centres admitted in one shape or other by all to exist[2]."

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[1] *Loc. cit.* p. 552.

[2] *Loc. cit.* II. 472–4.

It must be borne in mind that James' *Principles of Psychology* is rather a collection of essays written at different times than a systematic treatise. He gives no classification of the elementary states of consciousness and calls them all feelings.. He does not treat of feeling-tone or pleasantness-unpleasantness, but only of emotions.

(4) H. Höffding: "If it is true that we can distinguish between cognition and affection only by means of abstraction, and that every concrete state of consciousness is composed of elements of cognition and elements of affection, there will be no reason to expect that cognition and affection are dependent upon the function of different cerebral organs[1]."

"Since the development of affection is so closely connected with cognition, there will be just as little reason to expect a special localisation for it as for cognition. In the different forms of affection as in the different forms of cognition the same neurones may be thought of as functioning together, only in different degrees and in different combinations. Since affection appears to arise and to develop more slowly than cognition, it has to be assumed that it is represented physiologically by a greater extension of the neural process in the cerebral substance[2]."

"One is tempted to see in the antithesis of pleasantness and unpleasantness the expression of the antithesis of advance and regression of the vital process. It may be enunciated as chief rule that pleasantness indicates increased activity of life, higher and freer consumption of energy. ......Pleasantness stands therefore anyhow as the expression of increased life, pain as the expression of regression, as the harbinger of death[3]."

(5) Oswald Külpe: "During the preliminary interpretations which we attempted to give to the facts when reviewing them, we met already with a general conception corresponding to this theory, namely with the assumption that pleasantness and unpleasantness run parallel with the ratios of excitability of the central substance[4]."

"It cannot be denied that Meynert's theory of feeling—in spite of its partly very hypothetical foundations—has the advantage of making possible a more exact determination of the basis of pleasantness and unpleasantness, and of being able to formulate a simple relation to the results of the expression method and other facts described above[5]."

In his Report to the 6th International Congress of Psychology at Geneva in 1909 Külpe[6] gives a classification of the various theories of feeling. He refutes the correctness of all with the exception of that of Meynert of which he says: "The assumption of functional processes of feeling has led also here to the conception, that it must be states of nutrition which express themselves as pleasantness and unpleasantness. Meynert has given some more exact shape to this theory, the fundamental ideas of which appear plausible, whilst the details may prove to be premature."

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[1] *Loc. cit.* p. 338. [2] *Loc. cit.* p. 339. [3] *Loc. cit.* p. 344.

[4] *Loc. cit.* I. 280. [5] *Loc. cit.* I. 282.

[6] 6me Congrès international de Psychologie à Genève 1909. p 181 sq.

(6) Wilhelm Ostwald: "To the perfectly stationary current of energy, which is characteristic of normal life, corresponds a neutral 'feeling-tone,' where the occurring sensations do not appear as hindrance or furtherance of the regular course, but belong to its normal composition......On the other hand every furtherance of the energy current is experienced as pleasant, every hindrance as unpleasant[1]."

We see then from the examples chosen that all these authors, when they come to treat of the physical basis of feeling, have forgotten their premises, or, what is probably a more correct way of putting it, they are unmindful of the propositions that may be adduced as premises for the question with which they are dealing. The number of examples might have been increased, but these suffice, including, as they do, some of the best known names in the science of psychology. If we do not wish to admit the conclusion we shall have to reject one or the other of the premises, which few psychologists would be prepared to do.

II.

Before we turn to the question of evidence for a neural basis of feeling it appears necessary to say a few words about Pain. In ordinary language the term pain is applied to many different experiences. Thus we say we suffer pain when we have the toothache, when we cut or prick or burn ourselves. We speak of painful news or painful duties. A noise or a discord or an unharmonious colour-combination is often termed painful, and many similar instances will occur to every one. The pain of toothache, cuts, pricks or burns, etc. is often distinguished as *bodily* or *physical* pain, whilst *mental* or *moral* pain designates the unpleasantness occurring in the other examples cited above. But although in ordinary life such a picturesque description of the varied affective experiences is not without charm, it is an unpardonable laxity of thought if such a popular term as pain with its uncertain and varied connotation is adopted in an exact science as a *terminus*

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[1] *Loc. cit.* p. 388.

technicus without any further definition. It is doubtless due to this fact then that there exists in psychological literature a great amount of confusion. What all the experiences cited above as examples have in common is *unpleasantness,* and hence pain is very often, if not always, regarded as the acme of unpleasantness, that is as a feeling.

We trust that it will be evident from what follows that pleasure as indicating pleasantness, is not the opposite of pain, but the opposite of that which pain generally has in common with a good many other experiences, viz. unpleasantness. Pleasantness-unpleasantness, or pleasure-unpleasure, ought therefore to be the technical terms for the two qualities of feeling, the English equivalent for the German Lust-Unlust, whilst pain, in the proper psychological sense, is to be applied only to the sensation, the sense-experience, which is mostly, though not necessarily always, accompanied by a marked unpleasant feeling-tone.

It may be useful to review here some of the introspective results the author has obtained from an investigation carried out at the Psychological Laboratory, University College, University of London[1], to elucidate some of the most disputed points of the Feelings. In things psychical introspection is after all the court of final appeal. The judgments arrived at are analytical judgments and hence carry apodeictical weight, one cannot go behind them. It goes, of course, without saying that the investigation must be conducted with all the precautions demanded by scientific experiment and the introspection carried out by properly trained observers, known to be reliable. If, however, introspection is already a very delicate and exceedingly difficult matter when it concerns the cognitive content of consciousness, it becomes infinitely more so when the introspection is directed to the affective content. The feelings, unless of great strength—and in this case the experiment defeats itself if the feelings become objects of observation—are such a fleeting nebulous affair, so evasive when attention is directed to them, that results can be expected only from good introspecters, and even these require a long continued special training, as investigation has shown, and as is evident from a perusal of the following protocols. During the course of these experiments each subject had to introspect on something like 400 stimuli. A protocol was dictated by the observer immediately on the termination of each introspection and taken down by the experimenter. It is not necessary to give here a full description of the research, which will be published in due course, it must suffice to select some of the introspective results on the application of painful stimuli.

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[1] I may take this opportunity of thanking Prof. Spearman for allowing the work to be carried out in his laboratory and Prof. Carveth Read, Mr J. C. Flügel, Dr Ll. Wynn Jones, and Mr G. E. Phillips for having acted as subjects in this lengthy investigation.

Now it has been a very reprehensible custom with most investigators in this field to content themselves with giving only a summary of the results obtained, illustrated now and again by a quotation from the protocol. Such procedure is quite valueless and may even be misleading. Unless the protocol is given *in extenso* the results carry little weight, as the reader cannot form an opinion for himself about the experiment and the reliability of the introspection. The conclusions of the experimenter are, after all, merely individual opinions unamenable to any criticism. The protocol is the material out of which the conclusions are manufactured, and unless this material is made accessible it cannot be examined as to its value and the results cannot be criticized.

The experiments consisted of four groups of increasing difficulty, viz.

Group 1. Simple single stimuli.

Group 2. Pairs of stimuli of the same kind, but of different intensity and/or extensity and/or duration.

Group 3. Pairs of homogeneous stimuli (*i.e.* of the same modality) of different kinds.

Group 4. Pairs of heterogeneous stimuli (*i.e.* of different modality).

The *painful stimuli* were:

P 1. Bristle prick (diameter of bristle 0·4 mm. exerting a maximum pressure of about 32 gr.).

P 2. Pressure on deep tissues by means of a glass rod (diameter 7 mm.) with rounded ends.

P 3. Forceps pinch.

These painful stimuli were all applied to the back of the hands, successively in group 2, simultaneously in groups 3 and 4. The observers were four in number, all men, and all trained introspecters and all engaged in the teaching of psychology at University College.

SUBJECT R.

Group 1.

P 1. Slight PSn (= pain sensation); agreeable though not highly.

P 3. Unpl. (= unpleasant). It was a pinch, only slightly so. Part of unpls (= unpleasantness) was due to fear that it would increase. At first a Sn (= sensation) of contact and wrinkling of skin; unpl; then PSn rather sharp came out, which was unpl—possibly it excited a reflex of which I was hardly aware.

P 2. There was a contact Sn, then gradually a PSn, but it could not be definitely unpl as far as it went. Apprehension arose as to how much stronger it would be made, and this apprehension was unpl. Sn itself was as indifferent as a Sn can be. There was unpls in the experience afterwards that I could not form a definite judgment.

Group 2.

P 1 (i.d. < = varying in intensity and duration, first stimulus weaker and shorter than second). I think the first Sn in which P was slightly developed was on the whole pl (= pleasant). The second Sn in which P was slightly more intense and also sharper in quality was on the whole unpl. The P was of course connected with the tactile Sn and I should say that the tactile Sn was in the first case more unpl than the PSn. In the second case the tactile St (= stimulus) nearly ceased to be felt, it was masked by the PSn.

P 1 (i.d. <). First Sn agreeable, there was a sharp but mild PSn. The second one was a little stronger and of uncertain quality; then it was not uniform during its duration. After the beginning of the PSn there was a brief interval of contact Sn without P, then PSn rather more intense than before and decidedly unpl.

P 3 (i.d. <). I could discriminate in both experiments the Contact Sn from the PSn. The P, however, in the first experiment, as it became prominent, completely masked the contact Sn and became pure P of a considerable degree of intensity. The second experiment gave a stronger and more extensive contact Sn, but the P was duller, more extensive and less intensive, so that it never submerged or masked the contact Sn. The first experiment at its height was very unpl, it excited a strong reflex which was drawn off by knitting my brows. The second experiment was much less unpl and excited no perceptible reflex, but it was unpl, though much less so than the first experiment. I think that in some of these experiments I can detect an Unpls accompanying the general situation of being experimented upon which becomes most prominent in PSn. This general ft (= feeling-tone) of the situation is marked by attention to the particular St (= stimulus). I cannot say whether it influences the ft of the particular Sn. I have only just become aware of this.

P 2 (i.d. <). Both slightly unpl. The first hardly distinguishable from indifference, mixed contact and P, but in the first the P developed so slightly that it was hardly recognisable. The second became a little clearer and perhaps a little more unpl. The ft was very faint in both cases. The localization of the Unpls was complete. it coincided with the locality of the Sn.

T 6 (*i.e.*, expt with tactile St No. 6, viz., rough sandpaper). The first was unpl, it contained two Sns, rough contact which was slightly unpl, and intermittent PSns which were more unpl.

Group 3.

T 2–6 (T 2 = powder-puff, T 6 = rough sandpaper). No. 2 slightly agreeable. No. 6 mixed F (= feelings) attending two different Sn-qualities: a sort of scratching, *i.e.* a slight P, and slightly rough contact-Sns. The scratching Sns were on the whole pl. The contact Sns were nearly indifferent. The Pls of the scratching Sn seemed to be due to a comparison with the Sns from No. 2. It was as if No. 2 St had left a negative after-image of unpl quality and the scratching Sns obliterated it. This immediate comparison made without deliberation, merely automatic, seemed the chief ground of Pls of No. 6. I don't think the pl quality of the feeling-tone of the PSn was any different from the pl ft of other Sns, but it was more intense. The fs were distinctly localized with the Sns.

P 1–2. No. 1 at first unpl, gradually becoming less so. No. 2 only yielded pressure Sns for 5 to 6 secs., a dull PSn gradually developed in strong contrast with the poignancy of No. 1. As the Unpls of No. 2 became greater that of No. 1 grew less. Possibly this may be due to attention being directed to No. 2 rather than an actual change in ft of No. 1. No. 2 Sn seemed to spread beyond the point directly excited and the Unpls grew still greater and diffuse. It appreciably affected my breathing, making it, I thought, deeper, contrary to the usual report. The ft of No. 1 was much more localized than that of No. 2. I find it impossible to say whether the two unpl fs differed in quality. The total experiences of No. 1 and No. 2 were very different, but the difference of quality may be wholly referred to the difference in the Sns. The Sn and ft of No. 2 persisted for a good while after the St had ceased, very much longer than in the case of No. 1. I could be aware of the Sns simultaneously, but am not able to say that I could discriminate the fts simultaneously, this seemed to depend upon the transfer of attention from one Sn to the other.

P 1–3. Both unpl. No. 3 more unpl. I think I could compare the fts simultaneously, but I am not sure of it. The ft of No. 3 was more diffuse, *i.e.* less entirely localized than No. 1. It was nevertheless more definitely unpl, so that it was a relief to turn attention to No. 1. When this was fully accomplished No. 3 temporarily disappeared from consciousness.

P 2–3. Both unpl, No. 3 more so. As the PSn of the pinch developed it was hardly distinguishable from a prick, but more intense than the prick in former experiments I tried hard to compare them simultaneously but with very doubtful success. They were both chiefly localized. No. 3 more completely than No. 2. The Unpls of No. 3 was not the accompaniment of an intenser P, for No. 2 was more painful. The greater Unpls attached to the quality of the P.

Group 4.

P 2–G 2 (raspberry syrup). Both were fairly unpl. The gustatory Sn rather indeterminate, sweetish, but without other quality. The PSn was more definite and intense. The ft faintly disagreeable, but only faintly. I cannot say which of the 2 fts was the more unpl whilst the PSt was active, but the gustatory Sn still remains active (2–3 mins. after cessation of the PSt) and is now much more unpl than the P was at its maximum. As to the quality of the fts of the Sns I cannot compare them definitely enough to perceive any qualitative differences in them. The difficulty is to distinguish the fts from the quality of the Sns on the one hand and the quality of the motor reactions excited on the other hand. Allowing for these differences there seems to be nothing left except difference of duration and intensity.

T 5–A 6 (fairly rough sandpaper—a squeaking noise produced by turning the glass-stopper in the neck of a bottle)……The tactile scrape Sn was also mixed, a roughish contact with slight and irregular P factors. It was slightly agreeable. The element of pain seemed to give a sort of freshness to the total Sn. At first these PSns appeared pl, but further analysis raised a doubt whether this Pls was not attached rather to the excitement.

P 2–A 4 (tone of Galton whistle).......The pressure St as long as it is merely pressure is nearly indifferent. When P is aroused it is slightly unpl under the pressure then exerted. So far as I can compare the fts and distinguish them from the Sns they seemed not to differ in quality. Both are localized, the ft of the P in the hand at the point of pressure, the ft of the sound definitely in the ear with merely a reference to an objective St......The quality of the P from the pressure St is quite distinct from the quality of the contact Sn.

P 2–V 1 (a square piece of coloured paper). The colour was pl, a sort of confidence which it gives. The PSn unpl; by degrees it seemed to penetrate the hand so that it was experienced on the palm. Until it became very unpl and drew the attention very strongly I was able to compare the two fts better than usually; they were strongly contrasted, but not simultaneously. The difference of intensity was very great. The ft of the PSn was much more localized than the ft of the visual Sn. It excited much more tendency to motor action. The visual St seemed merely to hold the eye.

P 1–O 6 (smell of skatol). The olfactory Sn was at first slightly pl, but as it continued it became unpl and seemed to get increasingly so, so that if it had lasted longer it would have become offensive. The PSn was somewhat slightly unpl throughout. Contact Sn appeared at first, but was submerged by the PSn. It was very difficult to compare the fts of the two Sns. I could not say that they differed from one another as feelings. But as these expts go on I become less and less able to view the feelings as something distinct from the Sn on the one hand and the motor reaction on the other hand. I should say that the Unpls of the PSn bore the same relation to the Sn as the Unpls of the olfactory Sn bore to that Sn. A part of the difficulty of comparing the feelings simultaneously arises from the difficulty of appreciating the true character of the different Sns each for itself. Thus as to the olfactory St in this expt it was some time before I could make up my mind as to what it was (*i.e.* the real experience itself) and secondly whether it was agreeable or not. The P as it developed had a new modality; it was not a contact Sn of unpl ft. The unpl ft of the olfactory Sn at its height was much stronger than the ft of the P.

P 2–G 4 (taste of quinine hydrochloride soln). Both unpl, the PSn decidedly more so......The PSn was decidedly unpl and became increasingly so. Its modality was entirely different from contact. It called forth towards the end a decided desire to stop it. So far as the two fts could be compared, no difference of quality could be detected. Even with the PSn there was a slight admixture of satisfaction, at least at the beginning of it.

P 3–T 6 (rough sandpaper). Both unpl but the PSn much more so. In the contact Sn there was only a very slight admixture of P at the border, the edge of it. The Unpls of the contact Sn was not unmixed; there was a slight experience of satisfaction, agreeable. The PSn was increasingly unpl, more unpl than either the pressure or the prick Sn of previous experiments this afternoon. As to feeling-quality there was no perceptible difference between the two Sns, but only in intensity. P is a Sn of a different modality to the contact Sn and it is quite different from its ft.

SUBJECT F.

Group 1.

P 1. Tension (expectant attention); at first indifferent, then there rose quickly PSn of pricking character which was distinctly though not entirely unpl.—Sn of warmth.—F came in consciousness quickly and rose quickly in intensity, afterwards declining until a condition of practical indifference was reached. Thought "this is tolerable." An after-effect was quite as unpl.

P 3. First slightly pl, coolness—Pls went slowly—period of wondering what was happening—became aware of painful Sn, unpl from first—Unpls grew considerably with intensity—recognized a pinch—Unpls grew to high level—incipient motor tendencies and tendencies to shout.

T 6 (rough sandpaper). Period of adaptation, followed by rapid rise of unpl F. Then became aware of P mixed with pressure. P was intermittent. Unpls oscillated also—movement tendencies—fear or apprehension which became accentuated in the intervals—hoped it would soon end.

P 2. Tension, expectation, apprehension in fore period. I became aware of St, temperature and pressure Sns, coolness pl—vanished—pressure Sns became more intense and became unpl—slight degree of anger towards St—P which increased steadily in intensity—unpl from first—P much later than pressure Sn—tendency to movement.

G 3 (dilute acetic acid). First mere tactile Sn, then warmth which was pl,—changed to definite gustatory Sn, which was slightly unpl—became aware of pricking Sns, at first slightly unpl, becoming at intervals pl. Gustatory Sn returned to consciousness with prick and became highly pl at intervals. The whole had a tone about it which is best described as voluptuous—then prick became more unpl, although a pl element remained in consciousness—they appeared to be together, simultaneous or in very quick succession—swallowed.

Group 2.

P 1 (i.d. <). First: initial period of adaptation accompanied by apprehension—rapid rise of Unpls to fairly high level, only abated gradually together with Sn. Second: Unpls rose less rapidly and attained less high level and was declining at end. Thought that I was pleased that it was not worse, "feeling of relief" which was slightly pl. It really seemed to be present at the same time as the Unpls of the Sn.

T 5 (i.e.d. <). Second more unpl.—In first slight Unpls, which seemed caused by the rough and jerky character of Sn.—In second there were distinctly PSns, rapid pricks which were unpl, and the pressure Sn itself which was also more unpl, although of the same character. In addition there was an idea of the uncouthness and clumsiness of the Sn, which was also unpl.

P 3 (i. <). First St. The PSn arose rather slowly some time after the tactile Sn, continued to rise and with it the Unpls, which soon attained a very considerable degree. I noticed very marked organic and muscular Sns, *e.g.* holding breath and general tendency to shrink and also a tendency to shout or cry out. Also a peculiar emotional state in which both fear and anger played a part. In the second Sn both the P and the feeling arose more slowly. Fear more marked at first but disappeared towards the end giving place to anger. Tendency to shrink and cry out as before. The general ft very unpl.

T 6 (i.d.e. >) (rough sandpaper). First more unpl. In the first the tactile Sn seemed to be unpl in itself, and in addition to this Unpls there was also Unpls accompanying PSns which shot up very frequently and attained considerable height; they were of short duration.......

P 2 (i.d. <). Second more unpl. As regards the first one the P arose again some time after the pressure which was completely indifferent. The consciousness was unpl during this period owing to the apprehension of the P to come. Then the P arose very quickly and rapidly became very unpl. I noticed very strong organic Sns especially from the region of the neck and throat. These were also unpl. As soon as the St ceased I thought "it is over and it was not very bad" and this thought was pl. This pl thought co-existed with the gradually diminishing PSn. The P at this stage was still unpl, so there was coexistence of Pls and Unpls. At this stage the Pls predominated over the Unpls. I cannot now give an introspection on the second St—I quite definitely localized the ft in the spot where I experienced the P. I remember now about the second St; a passive kind of emotion, a sort of resignation. The second was on a whole very much like the first.

Group 3.

T 1–6 (badger-hair brush and rough sandpaper). ...Besides the pressure Sns there were from time to time Sns of P which were very distinctly unpl.

P 1–2. No. 2 more unpl.—Attitude very passive and spectacular, except at the beginning when there was a slight tendency to withdraw the hands.—The two Sns and their respective fs coexisted in consciousness throughout the whole period though there were fluctuations in their respective intensities. The intensity of the feeling always corresponded to the clearness of the Sn. The fs of both were localized and quite distinct and for a considerable period they were present together in approximately equal intensity, No. 2 being slightly more unpl. There was conscious the fact that No. 2 covered a much larger area and was localized more deeply. No. 1 was sharp and pointed, a prick Sn; No. 2 was dull and achey. The spatial attributes and the "being deep" and cutaneous respectively played a great part. The moments when the two Sns and their fs were held most clearly apart and most clearly coexisted were the moments of highest attention.

G 3–4 (dil. acetic acid and quinine hydrochloride soln). ...No. 3 acquired a burning character which seemed more P than taste; this was at some time pl, but afterwards became unpl again.......

T 2–5 (powder-puff and medium sandpaper). ...No. 5 sometimes gave a succession of pressure Sns which were somewhat unpl, and sometimes rather violent PSns which were extremely unpl. P was of prick-like character which rapidly rose to very high intensity and then suddenly and completely vanished. During the majority of these P periods No. 2 practically vanished from consciousness to reappear as soon as the P had stopped. During the pressure periods 5 and 2 coexisted and also their respective Pls and Unpls which were fairly clearly localized. Towards the end I made a voluntary effort to keep No. 2 continuously in consciousness, but in spite of this effort it vanished completely at the moment of the PSns.

P 1–3. No. 3 more unpl.—No. 1 entered consciousness first and was distinctly unpl; then No. 3 entered consciousness with a rush somewhat driving out No. 1.—No. 3 rapidly became very unpl, but No. 1 soon came back again into comparatively clear consciousness, and was still unpl. The two Unplss coexisted. When they were equally in consciousness No. 3 was distinctly more unpl, but in general the intensity of the feelings varied directly as the intensity and clearness of the Sns. At one point the clearness of the two Sns was about equal, but Sn 3 was much more intense, and at that moment the Unpls of No. 1 very considerably declined whilst No. 3 was intensely unpl. Both feelings were well localized.—No. 1 was a prick-like P, No. 3 an ache-like P.

P 2–3. No. 3 more unpl.—No. 3 first perceived. At first it was only a tactile impression which was indifferent, then a PSn arose which was very unpl. No. 2 was then perceived, but was more or less indifferent. No. 2 then became gradually more unpl and the two Unplss coexisted, though from time to time No. 2 would become indifferent again. At these moments No. 3 was more than usually unpl.—No. 3 was usually more prominent in consciousness as was also its feeling-tone. Only on one occasion did No. 2 become more unpl.—No. 2 seemed to be a much deeper and duller P, No. 3 was sharper and better localized. When attention went principally to No. 2 I found myself adopting a patient passive attitude towards it. No. 3 seemed much more intolerable and I had to inhibit the tendency to move the hands. At the end of the experiment there was a period when the two Sns were much less definitely localized and at this point the feelings also ceased to be localized or held distinct from one another. There was now only one feeling of Unpls very vaguely localized somewhere in the front part of the body.

Group 4.

P 2–G 2 (raspberry syrup). G first cold and slightly unpl. As taste developed it became decidedly pl. At this moment I first perceived the P which was unpl from the start. Then ensued a continuous but rather slow oscillation of attention between the two Sns. They were both in consciousness the whole time and both maintained throughout their respective feelings. The clearness and intensity of the feelings fluctuated simultaneously with that of the Sns. For brief moments the two Sns and their respective feelings were about equally prominent in consciousness. Each feeling was distinctly localized.

T 5–A 6 (medium sandpaper and squeaking glass bottle). Both unpl. The tactile became painful. There was a rapid oscillation of the attention between the two Sns and very little if any coexistence in consciousness, except once or twice, when the usual painful accompaniments of the tactile St were absent. The unpl feeling of the PSn was very distinctly localized and seemed to be on the extreme outer surface of the skin, a very sharp pricking P, extremely unpl. It rose very quickly to a very high intensity, and then just as quickly dropped out of consciousness. The tactile Sns were for the most part only present in consciousness when P was absent and were then slightly unpl. They were localized more deeply than the P. The auditory St was more unpl than the tactual but not quite so unpl as the P.......

P 2–A 4 (Galton whistle). Very passive and spectacular attitude during the greater part of the time. The attention fluctuated very slowly between the two Sns and they were both continuously in consciousness. For the most part the attention was distributed fairly evenly between them. The one or the other always predominated slightly. The P was for the most part slightly unpl. At one time the Pain as such ceased altogether, leaving only the tactile Sn......The Unplre from the auditory Sn was at one moment to some extent fused with that from the PSn.......

P 2–V 1 (square of coloured paper). ...The Pleasure seemed in some way to compensate for the P and to make it more tolerable. The P itself fluctuated in intensity and also in Unpls, sometimes it was the more predominant of the two Sns and yet was less disagreeable than at other times, when it was less predominant. But I think the Unpls corresponded closely to the intensity of the Sn. Towards the end the P grew gradually more severe, but at the same time the Pls from the visual Sn also became greater. Finally both feelings coexisted at a fairly high intensity. The visual Pls seemed connected to some extent with vague and diffused organic Sns and was itself vaguely if at all localized. The Unpls of the P was distinctly localized in the hand.

P 1–O 6 (smell of skatol). The P was always unpl as long as there was P; sometimes the P vanished and there was only touch left. This was as nearly as possible indifferent......The Unpls of the P coexisted with both Pls and Unpls of the smell. The feeling-tone of the P was localized in the hand and that of the smell sometimes definitely in the nose, sometimes in the throat or gullet. This was particularly the case when the smell was especially unpl and when the organic Sns were prominent. —The Unplre of the olfactory Sn was much more complex and so intimately bound up with the organic Sns, whereas the Unpls of the P was well localized. When I have analyzed any apparent differences of quality in the unpleasures of the smell and of the P there seemed nothing left but differences of intensity and local sign and extensity.

V 4–G 3 (square of coloured paper and dil. acetic acid). ...Besides the taste there was very often a pricking Sn and sometimes a burning Sn, these two seemed often fused and were for the most part distinctly pl. Both the Plre and the Unplre occasioned by the taste St coexisted with the Plre of the visual Sn. I could distinguish no qualitative differences between the two Plres, when they coexisted, except that the Plre of the pricking Sn was distinctly localized, that of the visual unlocalized. This pricking Sn seemed to have the same quality as an ordinary painful prick; it was in fact a pain Sn.

T 3–P 1 (touching with a piece of plush). There was for the most part a rapid alternation of attention between the two Sns, the periods of attention to the tactile Sn being usually longer than those to the PSn. There were, however, some periods when both were definitely in consciousness. On these occasions the PSn was only slight. The tactile Sn was pl throughout, it had a soothing character which seemed to be all the more welcome in contrast to the Unplre of the P. The PSn itself varied somewhat in character from time to time, being sometimes localized more on the surface than at other times. It was during these periods that it was often particularly unpl and would then seem to monopolize consciousness, the tactile Sn quite vanishing for a few short moments. Towards the end I experienced some anger directed against things in general. This was particularly strong in moments following upon rather more severe P than usual. The Unpls of the P seemed to depend partly on its character being more severe when it was localized peripherally and partly on the intensity of the Sn itself.

P 1–G 4 (sol. of quinine hydrochloride). Gustatory St first cold slightly unpl; taste developed gradually. At first indifferent, then increasing in Unpls. At this point P was first perceived, unpl from the start, and the Unpls rapidly rose to a high intensity and for a short time completely occupied consciousness. The gustatory Sn quite disappeared, then it came gradually back, the P receding at the same time. But the Unpls of the P seemed to diminish more than its purely sensory side, so it was often only very slightly unpl when, so far as the Sn itself was concerned, it was fairly prominent in consciousness. As regards the gustatory Sn the degree of Unpls seemed to correspond more closely to the clearness of the Sn in consciousness. As a whole the gustatory Sn was more unpl than the P, except at the first. It was also less well localized, though it was localized in the region of the mouth. The P was clearly localized at a point on the hand. The two Sns and their fts continued to fluctuate very slowly during the whole period, but after the initial disappearance of the taste both Sns and their feelings were continuously in consciousness. There were diffused organic Sns which seemed intimately bound up with the gustatory Sn. These organic Sns themselves were indifferent or only slightly unpl. On swallowing the quinine more or less automatic shaking of the head took place and the resultant Sns seemed in some way to alleviate the extreme Unpls of the taste. There is in the P some quality which is fundamentally different from the pressure, which usually accompanies it and which is not the same as the Unpls which also accompanies it.

P 3–A 6 (squeaking glass bottle). Both Sns continuously present to consciousness and both were always unpl. The P was as a rule the more unpl, except once or twice when the auditory Sn became particularly intense and it became more unpl than P. The P was very well localized and contained the prick quality as last time, and sometimes there seemed also to be present a heat Sn which could just be analyzed out of the rather complex whole. The unpl feeling belongs to this whole and I could not attach it or any part of it to any of the constituents of this whole. In connexion with the P there was present during the greater part of the time a sort of mood of resignation which somehow seemed symbolized in the passivity of my left hand. This mood would, however, be broken in upon from time to time by a slight emotion of anger which seemed directed against the sound, the irritating part about the sound being that it took place while I was enduring the P. There were very diffused organic Sns throughout, but these were quite in the background of consciousness and had no marked feelings. The anger was distinctly unpl and its Unpls was distinct from that of the two Sns though all three Unplres coexisted. The resignation seemed as nearly as possible indifferent. I could not distinguish any qualitative difference between the various Unplres, that is not analyzable into some difference of the attributes of the feelings or the attributes of the Sns.

P 2–V 6 (coloured square). Both unpl. The two Sns were in consciousness the whole time and during the greater part of the time their respective feelings were quite distinct but coexisted. Towards the end the sensations and their feelings seemed to some extent to fuse into a total unpl situation in which there was no longer any clear discrimination between the two Sns and their feelings. The attitude was very spectacular throughout and there was towards the end an incipient mood of resignation. The colour was unpl in itself and did not call up any associations. There was, however, to begin with, a very slight emotion of disgust accompanied by very slight Sns or images of nausea. The P was fairly well localized and there seemed to be two constituents: a brighter sharper P like the prick Sn before and a duller P which was localized more deeply and which seemed to surround the central prick. I could also detect thermal elements, I think, both heat and cold. The feelings seemed to attach to the whole complex Sn. While the two feelings coexisted I could not distinguish any difference in quality between them, but the Unplre of the P was more intense.

SUBJECT J.

Group 1.

P 1. Slight Sn of P, slightly unpl (second application). There was a certain amount of P, ft was slightly unpl.

P 3. Strongly unpl. P.

P 2. There was P on pressure on bone, ft unpl.

Group 2.

P 1 (d.i. <). Both unpl; both Sns of P; second stronger. Quite simple to analyze compared to gustatory. I think it is quite easy to separate feeling from Sn.

P 3 (i.e.d. >). Both unpl, distinctly so, both P; not conscious of difference of duration; difference of extensity asserted itself only after a while; first only slightly more intense.

P 2 (i.d. <). Both painful, unpl, second decidedly more so (subject cried out). PSn and ft differed about equally in both Sns. P seemed to be due to two things, viz. skin Sn, and bone Sn.

Group 3.

P 1 . 2. Both unpl, No. 1 decidedly more so, I was conscious of distinct difference of quality; No. 1 was prick, No. 2 was pressure on bone. No. 2 had extensity. I am not so sure about No. 1, it seemed to be a sort of pinching or piercing at a point.—The fts did not fuse, there seemed to be oscillation. Ft of No. 1 predominated.

P 1 . 3. At first I had a tactile Sn which was slightly unpl, then I had a distinct unpl ft which was at once attached to No. 3 which was now P on my left hand. I was conscious of a slight pricking Sn on my right hand at certain moments, but the ft, as far as I can judge, was neutral. I recognized this as a PSn also.

P 2 . 3. At first I had the slight unpl ft due to No. 3, and then I had another unpl ft due to No. 2. Then later on I was conscious of an unpl ft and it was not localized either in the right or left, in fact both Sns in consciousness were only marginal, but the ft was unpl, *i.e.* I felt uncomfortable due to sti which were not localized with accuracy, nor the one differentiated from the other, except at points when either the one or the other became dominant. At that time the ft proper to that Sn came up.

Group 4.

P 2 . G 2 (raspberry syrup). At first pl gustatory Sn and then when I sensed the pressure the pl ft vanished and there only remained the unpl ft of the P, except for one moment when I had the pl ft due to G. At that moment the unpl ft due to the P had gone and it immediately came back when simultaneously the pl ft of G vanished. There was never any coexistence. I could say this time the Unpls was here in my hand. The pl ft of G was less definitely localized in the mouth, it was more diffuse.

P 2–A 4 (Galton whistle). At the start I was conscious of the two Sns, but I had no fts. After a while I had a slight unpl ft due to the P and the Unpls increased with the time, and the more it increased the less became the auditory Sn. At certain moments the auditory Sn was only in marginal consciousness. I was also conscious of a state of doubt whether the auditory Sn was not a little pl at times. The P was localized quite plainly in the hand and the auditory Sn was localized in space. The Unpls of the P was localized in the hand.

P 2–V 1 (coloured square). First very slight pl ft due to the colour. I was then conscious of the two Sns but the ft due to each became almost neutral. Further they seemed to merge into one ft which was not localized. Then I was conscious of a PSn which had a slight unpl ft, but the Sn of blue was always in consciousness and there was no ft attached to it. At no time was there coexistence of ft. I localized the Unpls of ft in my hand.

P 1–O 6 (smell of skatol). The fts were unpl. I cannot say that they were coexistent at any time, but rather that they alternated. The prick Sn and its ft were more sharply localized than the olfactory Sn and its ft, although the latter were also localized fairly well. After a time there was an unpl F which was not localized; it seemed to be attached to the complex of the two Sns. I could not discover any qualitative difference in the two fts, nor is it possible to say which was the more unpl.

P 1—T 3 (touch with a piece of plush). At first I had a sharp PSn. The ft attached to it was unpl and distinctly localized. At the same time the ft seemed to affect me as a whole. The ft due to the tactile Sn was at first neutral. Later the velvety character had a pl ft. I was not conscious of the fts coexisting, but quite conscious of the alternation between the two.

P 1–G 4 (quinine). Both ft decidedly unpl. The P st gave a tactile-pain complex whose ft was unpl. All the fts were localized with the Sns but I also had a diffuse feeling which was also unpl. The gustatory st was tactile, thermal and gustatory, but it was the gustatory Sn alone which was unpl. This ft was also localized with its Sn. I cannot say whether the ft coexisted.

SUBJECT P.

Group 1.

P 1. First rather pl then unpl, as pressure increased slightly unpl. I could distinguish the Sn from the ft. St did not strike me as a prick at all, more like a pressure.

P 3. First ft of pressure Sn (seemed to be pressure downwards) which was not unpl. As pressure increased it became unpl. As the ft became markedly unpl Sn of P arose. The unpl ft preceded the PSn. I can analyze the unpl ft from the pressure Sn, but not from the PSn. No images.

P 2. I seemed to be able to distinguish three kinds of Sns: (1) tactile touch proper, changing into (2) pressure; (3) Sn of P which I seemed to be able to localize in bone. Again I had change of ft from neutral to unpl. Distinct impulse to withdraw. The same difficulty as before, I cannot distinguish PSn from its ft. Some visual images.

Group 2.

P 1 (i.d. <). The ft of the first was at first pl and continued till nearly the end of the stimulation, when it became slightly unpl. The Sn changed from pressure to P when the ft changed. The ft of P seems hardly distinguishable from the Sn itself. In the second st the first ft was pl pressure and changed very rapidly to unpl and change in ft seems to coincide with arrival of PSn. I think I can here distinguish the ft from the P itself. Strong impulse to pull my hand away.

P 3 (e.i.d. <). In the first st I could distinguish the pressure from the PSn. The pressure Sn seemed to be neutral. The PSn arose later, and was strongly unpl accompanied by contraction of thigh muscles, no visual image. The second st: I got no PSn at all, only pressure which was slightly pl. Here again I had a chance of comparing two fts, *i.e.* the unpl ft of the first st had not yet died away and the second ft alternated, I could not grasp them together in consciousness.

P 2 (i.d. <). The first st: I could distinguish the touch from pressure Sn, they were both neutral; visual image. The second st: I can easily distinguish the pressure from the PSn. The pressure Sn was slightly pl which turned to unpl when the PSn arose. The two fts alternated rapidly, they did not coexist. Impulse to draw hand away.

Group 3.

P 1 . 2. No 1 very unpl, No. 2 not so unpl. In No. 1 the PSn was present the whole time and I could only at intervals detach the PSn from the tactile Sn. In No. 2 the PSn was not present the whole time and I could more easily distinguish it from the pressure Sn. The two PSns were not in consciousness together as far as I can judge, that belonging to No. 1 seemed to swamp that belonging to No. 2. In No. 1 I do not think I succeeded in disconnecting the PSn from its ft. In No. 2 this was more easy. With No. 1 I had constant impulses to withdraw the hand.

P 1 . 2 (repeated on another date). No. 1 unpl, No. 2 if anything was slightly pl. I found I could attend to the two Sns simultaneously, if I attended to Sn No. 2. If I tried to distribute the attention evenly over the two No. 1 became predominant and No. 2 disappeared. Only at the beginning, before the PSn became too intense, could I discriminate the pressure Sn from the PSn in No. 1. With No. 2 I had no PSn and only momentarily could I distinguish any ft. In No. 1 I failed to discriminate between the PSn and its feeling-tone, they seemed so bound up together. With No. 1 there were impulses to pull the hand away.

Special experiment. Stimulus P 1 was applied with gradually increasing force. At first there was a pressure Sn with a slight pl ft. As the pressure increased the ft became unpl, but it was not a PSn. Then the pressure increased still more, there arose *suddenly* the PSn which was then the chief thing in consciousness and I could distinguish no pressure Sn. I can say from this that the PSn had a definite modality of its own, quite distinct from an unpl pressure Sn.

The investigation from the protocols of which the above extracts are given is not yet completed. Yet as far as the results have been made available they are of great value here. We may from this introspective evidence draw, among others, the following conclusions.

(1) *Pain is distinguishable from contact or touch sensations.*

Subj. R. states this twice on p. 439 (group 2, P 3 and T 6). Subj. F. states this implicitly throughout his protocol. Subj. P. makes two explicit statements to that effect on p. 449 (group 2, P 2 and P 3).

(2) *Pain is a sensation of a definite quality or modality of its own.*

Statements to this effect are made by subj. R. on p. 441 (group 4, P 1 . O 6–P 2 . G 4–P 3 . T 6) and subject P. on p. 449 in "special experiment."

(3) *The sensation of pain is distinguishable from its feeling-tone.*

This is explicitly stated by subj. J. on p. 447 (group 2, P 1) and by subj. P. on pp 448–9 (group 1, P 1 and group 2, P 1). The other subjects R. and F. assert this implicitly throughout the protocols.

(4) *Pain is not the acme of unpleasantness, since there are experienced other feeling-tones of other sensations which are more unpleasant than that of pain.*

Statements to this effect by subj. R. are found on p. 439 (group 1, P 2), p. 441 (group 4, P 2 . G 2), p. 441 (group 4, P 1 . O 6) and subj. F. on p. 446 (group 4, P 3 . A 6).

(5) *The sensation of pain may have a pleasant feeling-tone.*

This has occurred to subj. R. as recorded on pp. 438–9 (group 1, P 1, group 2, P 1 twice), p. 440 (group 3, T 2–6), p. 440 (group 4, T 5 . A 6). Subj. F. p. 442 (group 1, P 1 and G 3); p. 443 (group 3, G 3–4); p. 445 (group 4, V 4 . G 3); subj. J. p. 447 (group 3, P 1 . 3).

(6) *The feeling-tone is mostly localizable.*

With tactile, pain, olfactory and gustatory sensations this is almost invariably the case with subjects R. and F. Subjects P. and J. are unable to localize the feeling-tone at the beginning of the investigation, but as the training proceeds statements to that effect become more and more frequent.

Combining the results (1) to (5) we may express them as follows: *Pain is not a feeling-tone, not the acme of unpleasantness. It is a sensation of a definite modality whose feeling-tone is mostly unpleasant, but which may be neutral or even pleasant at times.*

Let us now turn from the results of introspection carried out under experimental conditions to those obtained from ordinary observation as well as to theoretical considerations.

First, we meet not unfrequently with pain where the feeling-tone is anything but unpleasant. Thus, *e.g.* in periodontitis, when we gently and for a short time press upon the diseased tooth we produce a sensation which is decidedly pain, but the feeling-tone of which is as decidedly pleasant. But if the pressure is prolonged or increased the feeling-tone becomes highly unpleasant, whilst the quality of the sensation remains unchanged. Similar conditions prevail in the case of perniones (chilblains). If we, even during a period where no unpleasant itching sensations are experienced in the affected part, exert a pressure upon it, this pressure is recognized distinctly as pain which is distinctly pleasant. It might here be argued that the pain sensation produced by pressure on the diseased tooth or on the chilblain is not in itself pleasant, but that it is the relief caused by the pressure that possesses the pleasant feeling-tone. To this may be answered that no relief is given since the tooth is pressed harder against the inflamed periosteum. In the case of chilblains the pleasant pain-sensation may be produced at a time when there is no other unpleasant sensation coming in from the affected part. But if there are unpleasant sensations derived from the chilblain the sensation of the added pressure, which by itself would be unpleasant, produces a pleasant sensation, *i.e.* the sum or the combination of two unpleasant sensations is a pleasant one. The same remarks apply to prurigo and scratching. In maniacal states pain is often described as pleasant. Thus Ziehen mentions a maniacal patient with a carious tooth who spoke of her "divine toothache[1]."

To this class belong probably those cases where people, under the stress of some great excitement, mostly religious, inflict upon themselves terrible injuries or mutilations, or suffer them to be inflicted, without apparently suffering any pain in the ordinary sense of the word. As examples we may cite among others the Buddhist Fakirs in India and the Flagellants in Europe during the thirteenth and fourteenth centuries. That form of algolagny known as Masochism may be counted here too. All these cases become comprehensible if we assume that whilst the quality of the sensation has remained constant, its feeling-tone has changed and become pleasant. It might be contended that a psychical analgesia would explain these cases equally well, but to this there is the objection that the undeniable excessively strong conative tendencies in every instance can find a solution by a highly marked feeling-tone only.

That it is a great mistake to confound pain with unpleasantness, to regard it, in fact, as the acme of unpleasantness, is further evidenced by the researches of v. Tschisch[1] who inhaled nitrous oxide or injected cocaine subcutaneously. "Small doses did not affect sensations, cuts and needle-pricks were sensed, they did not produce pain but only unpleasantness." This statement doubtless means that the sensation of pain was abolished and that only sensations of cutaneous touch with their accompanying feeling of unpleasantness remained.

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[1] "Gottliche Zahnschmerz." Ziehen. *Psychiatrie*, 1894, p. 16.

Head and Thompson[2] state: "When a strong interrupted current is applied to the foot, the patient cries out with pain and withdraws the leg. Suppose, however, that the lower extremity is totally insensitive to painful stimulation of all kinds, in consequence of an intramedullary lesion, the current, even when of excessive strength, cannot cause pain. But so long as tactile sensibility remains perfect, he will complain bitterly of the discomfort caused by this form of stimulation.

"Careful experiments with G. G. (No. 3, p. 656) and W. M. (No. 5, p. 667), both of whom were unusually trustworthy and willing patients, showed that the movement of withdrawal seemed to be almost as violent, when the current of known strength was applied to the analgesic as to the normal leg. G. G. said the sensation produced was a kind of exaggerated tickling more unpleasant than pain. Both these patients were firm in their assertions that the sensation was not painful; and yet an observer watching their behaviour would suppose they were undergoing intolerable pain. This reaction was not evoked on the analgesic side by a purely painful stimulus such as the prick of a pin.

"Evidently stimulation with a strong interrupted current normally causes pain, which rising to intolerable limits obscures all other sensations. But, if the painful impulses are interrupted by an intramedullary lesion, the tactile side of the stimulus is revealed, and, when the current is strengthened, can produce a sensation painless, but intensely disagreeable."

There are also certain sensations, "Stichempfindungen," of which we shall speak when we come to deal with pain-nerves, and which we shall endeavour to show to be pain-sensations of feeble intensity and often of a neutral or pleasant feeling-tone (p. 460).

We may in our judgment as to the nature of pain also avail ourselves of the following great difference between sensation and feeling. Introspection reveals that the difference between sensation or perception and its memory-image is very great indeed, and we need not go here into the details of such difference which have been exhaustively discussed in psychological literature. If we look at, say, a red rose and perceive it, and after a little while ekphore its memory-image, we note immediately how unlike, in its likeness, this memory-image is to the original perception. But if we have introspected also with regard to the affective side of the sense experience and noted the very pleasant feeling-tone we find, if we try to ekphore this feeling-tone with the memory-image and to attend to it, that there exists nothing on the affective side to correspond to the memory-image on the cognitive side. The feeling-tone of a memory-image, or its affective side of consciousness, is a fresh feeling-tone or emotion. Külpe[1] speaks thus about the matter: "We have divided the sensations into two large groups, viz. peripherally excited ones and centrally excited ones, and have found that this distinction is not unimportant for the state of psychical affairs, but rather, under normal conditions, well founded throughout our inner experience. With feelings, on the other hand, no deep difference can be established between peripherally and centrally excited states. Especially the centrally excited feelings are generally not inferior to peripherally excited ones as regards vividness, and are therefore able to compete with them successfully for the command over the will." If we examine now pain in the light of this, we find that it behaves exactly like a sensation. The memory-image of an experienced pain stands to that pain in exactly the same relation as the memory-image of a colour, or sound, or smell to the respective actual psychical experience, whilst the affective side of pain behaves like the affective side of any other sensation.

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[1] "Der Schmerz," *Zeitschr. f. Psychologie und Physiologie d. Sinnesorgane*, 1901, Bd XXVI. 14–32.

[2] "The Grouping of Afferent Impulses in the Spinal Cord," *Brain*, 1906, XXIX. 644–5.

How confused, however, the ideas of some authors are upon this point is instanced by the following quotation from Ribot[2]: "Moral pain is at first attached to an extremely simple representation, to a concrete image, that is to say, to an immediate copy of a perception. It may be defined as the ideal reproduction of physical pain. It requires only one condition, memory. The child who has had to swallow a nasty medicine, he who has had a tooth drawn, when they have to begin again, experience a pain which cannot be called physical, since it is attached to a mere image. It (the pain) is a feeble copy, an echo, of it. One may say in the language of mathematicians that, in this case, moral pain is to physical pain as image is to perception." Ribot calls the unpleasant feeling-tone accompanying the sensation of taste of a nasty medicine a physical pain and puts it into the same category as the sensation of pain, due to the extraction of a tooth. He fails to see that what is experienced with the memory-image of the taste of the nasty medicine or of the pain of tooth-extraction is a new occurrence of feeling-tone, a fresh emotion—of course of much less intensity than the feeling-tone or emotion attached to the original sense-experiences—and ends by calling it moral pain. A fresh confusion to the already confused use of these terms.

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[1] *Grundriss der Psychologie*, 1893, p. 231.

[2] *La Psychologie des sentiments*, 1908, p. 44.

Here is also the place for refuting a contention frequently advanced in order to show that pain is unlike a sensation, namely, whereas the cause of a sensation is, so to speak, projected into the outer world, *i.e.* it is interpreted by the individual as a property of the external world, this, it is said, is not so in the case of pain. The light appears inherent in the candle flame or the star, the sugar is sweet, and the rose smells as it does, but the knife or the needle does not look painful. But surely this statement is not in accordance with facts, for first there are sensations that are not projected into the outer world, and secondly there are cases where painfulness may be attributed to the object as much as sweetness to the sugar, or sound to the bell. The person who has suffered repeatedly from skotoma will no longer project the oscillation into the outer world. The sum of his experiences has shown him that the phenomenon is subjective and it will then always be apperceived by him as such. The same remarks apply to vertigo, tinnitus, formicatio, etc. The author who has experimented for years on the after-effect of seen movement[1] has long ago ceased to project the pseudo-movement into the outer world, but experiences it as a subjective phenomenon. Our outer world is an empirical structure and remains an outer world only as long as there is no contradiction in the sum total of our experience. The sufferer from skotoma looks the first time upon his hand and sees his skin in writhing movement, but there is no sensation from that part of the skin, and touching it with a finger confirms that the skin is motionless. A few repetitions of this experience and the objectivity of the movement will have ceased to suggest itself. As to the second point we have to deal here merely with a question of *acquirement of meaning*. The ice looks cold or the gong as having a soft sound because we often found them so, and to the person who has frequently submitted to the extraction of teeth without an anaesthetic the dentist's forceps look doubtless painful. Bill Sikes' dog would probably project as much painfulness into his master's boot as savouriness into a bone. "The unpleasantness of toothache," says Titchener[1], "is far more personal to me than the pain of it. The pain is in the tooth, the unpleasantness is my discomfort."

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[1] "On the After-effect of Seen Movement," *British Journal of Psychology, Monograph Supplement*, No. 1. 1911.

Hugo Feilchenfeld, who does not hold quite our views, expresses himself as follows: "By pain we understand the elementary and therefore no further definable body-sensation. Peculiar to it, in opposition to the other sensations, is that it is not objectified; in opposition to the feelings, that it is not excentred[2]." And again: "Pain is localised, pleasantness-unpleasantness is not localised...... Sensations are objectified and projected into the outer world; pain is excentred and localised; feelings are neither objectified nor excentred[3]."

Now this statement is absolutely wrong. It is the outcome of theorizing from preconceived ideas, and not the result of independent and unbiased introspection carried out under experimental conditions. We have only to refer to the few introspections quoted above to find numerous instances where the feeling-tone is localized as definitely as the sensation. Without giving further quotations from the protocols of the author's investigation on the feelings we may state that with tactile stimuli almost invariably the feeling-tone is localized with the sensation at the place of stimulation. The feeling-tone of the gustatory sensations is in the mouth and of olfactory sensations it is generally in the olfactory chamber of the nose. The feeling-tone of auditory sensations is localized in the ear or the head or sometimes referred to the stimulus in space. In the case of visual sensations the statement is that the feeling is diffused and sometimes it is localized with the stimulus in space.

Again, we may examine the question whether pain is a sensation or a feeling by Külpe's criterion as given in his masterly paper[4] where he states that feelings are: (*a*) elementary contents of consciousness, (*b*) pleasantness and unpleasantness, which are:

(*A*) distinguished from *sensations* by the fact that
  (1) they are not in a specific manner dependent upon sense organs,
  (2) they do not leave ideational residua;

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[1] *An Outline of Psychology*, 1902, p. 104.

[2] Hugo Feilchenfeld, "Über den Blendungsschmerz," *Zeitschr. f. Sinnesphysiologie*, 1907, Bd XLII. pp. 313–48.

[3] *Idem*: "Uber das Wesen des Schmerzes," *ibid.* pp. 172–91.

[4] "On the Psychology of Feeling," *Rapport au 6me Congrès international de Psychologie*, Genève, 1909, pp. 181 *seq.*

(*B*) distinguished from *ideas* by the fact that
  (1) they are not reproducible,
  (2) they do not stand in any associative relations, properly speaking, to each other or to other ideas;
(*C*) distinguished from *thoughts* by the fact that
  (1) they are not knowledge of objects,
  (2) they are not known objects.

Now pain is:

(*A*) (1) Specifically dependent upon sense organs. These sense organs are now generally held to be in the skin the numerous naked interepithelial nerve-fibrils. We shall presently revert (p. 457) to this matter and meanwhile turn to the next distinction.

(*A*) (2) Pain, unlike the feeling of unpleasantness, leaves a distinct ideational residuum, for we may have as distinct a memory-image of any particular pain as we may have of the sound of a bell. As to

(*B*) (1) the ekphory of the memory-image of a pain, and its absence in the case of feeling, this has been referred to already above (p. 453).

(*B*) (2) Pain *does* stand in associative relation to other ideas. The dentist's forceps will ekphore a memory-image of the pain previously experienced at a tooth-extraction.

(*C*) (1) We find that pain *does* impart knowledge of objects, as *e.g.* that our boot is tight, and

(*C*) (2) that pain forms part of known objects, *e.g.* a corn on the toe.

We see then also that by every one of these criteria pain is not feeling, but sensation, forming ideas and other cognitive contents of consciousness. That the question has perplexed many authors can be shown by their inconsistency of its treatment, not at various periods of their career but in the same book or paper. Thus Külpe[1] states on p. 92 of his Outlines: "What is specific of pain is apparently not the never wanting sensation quality, be this quality excessive heat or great pressure, or a shrieking sound, or a dazzling light, but the unpleasantness of which pain is considered to be the highest degree. The new quality which in pain is added to the sensations of the cutaneous sense is therefore probably not a special quality of the latter, but a *feeling*[2] which may arise by the excitation of all sensory nerves." Compare with this the statement on p. 248: "There may, however, also be a possibility that we have to recognize in pain, besides the unpleasantness that is contained and expressed in it, a special class of *sensation* that may be produced by the excessive stimulation of every sensory nerve. Unpleasantness would then evidently not be the vehicle of those qualitative differences but the special *pain sensation*[1] which combines with it in pain."

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[1] *Grundriss der Psychologie*, 1892.

[2] Gefühl. The italics are ours.

Another example is furnished by Richet[2]. At the beginning of his article he says: "It is a mistake to put pleasure as the opposite of pain, for it is difficult to mark the boundary of physiological pleasure, whilst pain is quite definite. The pain of a cut is something precise and net. As to pleasure, on the contrary, all is obscurity and confusion. The pleasure of the sexual act is the only acute and really physiological pleasure." Whilst towards the end he makes the following statement. "Every disagreeable excitation, every unpleasant (pénible) feeling is a pain: a discordant sound, a fetid smell, a dazzling light, a bitter taste are perceptions which may with perfect right be called painful."

Strong's[3] criticism may well be cited here. The theory which looks upon the feelings as aspects, or sides, or moments, he calls the "aspect theory" which is identical with Marshall's[4] "Quale Theory." He says, "touch, pain, heat, cold, are all independent one from another." He refers to Wundt, Lehmann and Külpe, who all hold pain to be maximal unpleasantness, but who all admit that intense pain changes the quality of a sensation which becomes reduced and eventually abolished. The statement reflects actually the facts and is incompatible with the aspect theory. At one extreme all is cognition and no feeling, and at the other extreme all is feeling and no cognition. "Can the intensity of a sensation, he asks, cover up and conceal the quality (*i.e.* pain)? No more than the breadth of a smile can conceal the face. The aspect theorist may be said to make the same mistake as a person who, viewing the series of colours between red and yellow, should ascribe the yellow apparently visible in the orange as a sort of tone, and the red visible in it as the sensational basis to which this tone is attached."

We may now with advantage return to the statement made on p. 456; that pain is specifically dependent upon sense-organs. It is at present generally held that these sense-organs, as first pointed out by v. Frey, are the numerous naked interepithelial nerve-fibrils, "...but these pain-endings in the skin," as Sherrington[5] puts it, "seem almost equally excited by stimuli of such different modes as mechanical, thermal conductive, thermal radiant, chemical and electrical. That is, they appear *anelective* receptors. But it is to be remarked that these agents, regarded as excitants of skin-pain, have all a certain character in common, namely this, that they become *adequate* as excitants of pain when they are of such intensity *as threatens damage to the skin*...they may be regarded as a group of excitants which has in relation to the organism one feature common to all its components, namely, a *nocuous* character." And "Further, the reflex they excite (i) is *prepotent*, (ii) tends to *protect* the threatened part by escape or defence, (iii) is *imperative*, and (iv) if we include psychical evidence and judge by analogy from introspection, is *accompanied by pain*[1]." As evidence that these free-ending fibrils function as the receptors of pain-nerves is frequently cited the fact that the cornea and conjunctiva give rise to pain only, without tactile or thermal sensations, and that these tissues do not contain any other nerve-endings but free neuro-fibrils. The correctness of this view has, however, been attacked[2]. A more suitable example is, I believe, the dentine. Here J. Howard Mummery[3] has shown lately that free-ending neuro-fibrils exist, and it has been confirmed by T. Deppendorf[4]. From the dentine no other but pain-sensations can be elicited.

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[1] Italics are ours.

[2] "La douleur," *Revue philosophique*, 2^{me} année, tome IV. pp. 457–81.

[3] "The Psychology of Pain," *Psychological Review*, 1895, II. 329–47.

[4] *Pain, Pleasure, Aesthetics*, 1894.

[5] *The Integrative Action of the Nervous System*, 1909, p. 227.

Perhaps the strongest objection to admitting the existence of special pain-nerves is the one advanced by Rivers[5] who looks upon the above mentioned neuro-fibrils as the end-organs of common sensibility, and pain as the psychical concomitant of excessive stimulation of these end-organs. The reason given for this view makes it at first appear quite feasible. Rivers states it thus: "It may happen to a man to suffer pain in a particular region or tissue of the body, once only in the course of his lifetime, or possibly not even once, nay we may suppose that in this or that region or tissue pain is felt once only in one individual among a large number of persons. If we suppose that pain is not as suggested above an excessive phase of something which is continually going on in a lower phase, but a something by itself quite distinct from all other sensations, we are driven to conclude, since such a sensation must have a special mechanism, including special afferent nerve-fibres to carry it out, that in the case in question such a mechanism of pain has been preserved intact but unused through whole generations in order that it may once in a while come into use, which is in the highest degree improbable. This difficulty disappears if we suppose that the constantly smouldering embers of common sensibility may be at any moment fanned into the flame of pain. We may conclude then that the skin in common with other tissues possesses common sensibility, and that when this is excited in excess, so as distinctly to affect consciousness, we call it pain." The force of the contention is evident, that in the course of evolution no system of organs could have been developed and maintained itself unatrophied where it does not frequently function. It may appear improbable, in the present state of our knowledge, that there should be any special pain-nerves developed in every part of the organism when many of these do not function in one individual in a million, or for many generations. There is, however, this vital objection against Rivers' theory.

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[1] *Loc. cit.* p. 319.

[2] *Cf.* W. H. R. Rivers in Foster's *Textbook of Physiology*, 6th edition, 1900, pt iv. 1541.

[3] "On the Distribution of the Nerves of the Dental Pulp," *Phil. Trans. Roy. Soc.* London, Feb. 1912, CCII B. 337–49.

[4] "Beiträge zur Kenntnis der Innervierung der menschlichen Zahnpulpa und des Dentins," *Deutsche Monatsschrift fur Zahnheilkunde*, Sept. 1913, Heft 9. 31$^{\text{er}}$ Jahrgang, pp. 689–719.

[5] *Loc. cit.* p. 1540.

The cognitive content furnished to our consciousness by the common sensibility or body sensations is very meagre, generally probably almost nil or only forming a fringe or a background. The affective content of consciousness is probably also mostly very slight and marginal. Frequently, however, this affective content may become highly pleasant or unpleasant and may give rise to states of consciousness when feeling only appears to be the content (*vide* p. 428), a cognitive content being practically absent or constantly changing and evidently independent of, and without influence on, the affective content. Now the unpleasant feeling may become very great, unspeakable, as *e.g.* in approaching collapse or severe sea-sickness, yet it is not pain. Or the body sensations may become very great and powerful as for instance those that give rise to desire for defaecation in diarrhoea, or those in vomiting. The increased intensity of the sensations or the unpleasant feeling is recognized, as it is in a dazzling light or deafening noise, but without the trace of actual pain in the one or the other. On the other hand pain may be present and be distinctly recognized as such, yet it need not be very strong or very unpleasant.

We shall therefore have to discard Rivers' argument that the interepethelial fibrils are the nerve-endings for common sensibility, the excessive excitation of which we call pain. In this we are justified by the following consideration. The pain-spots give rise, when gently stimulated, to sensations called "Stichempfindungen," sensations of prick, which are said to be entirely free from pain. Thunberg[1] says this about them (the italics are ours): "The *freedom* of these sensations *from pain* is excellently demonstrated by the fact that they most certainly constitute an important component in gustatory sensations, which are excited by several of our condiments, *e.g.* mustard and pepper, and with intensities where these sensations are still pleasant. Also carbon-dioxide (in sparkling beverages) and alcohol may produce agreeable compound sensations, where these prick-sensations form a component." Now here too, we maintain, there is faulty analysis, again due entirely to the baneful influence of our ill-defined terminology. These gentle "Stichempfindungen" which "*are free from pain,*" and the component of the compound taste sensation which is found in mustard, pepper, and sparkling beverages are all sensations of the *very same quality,* of the same modality, as are those sensations which are ordinarily called pain-sensations; their intensity is feeble, and when it is strong they constitute *Pain* of ordinary language. If it is not expedient to banish the term Pain entirely from the psychological vocabulary, we must at least always bear in mind that *Pain,* as a psychological *terminus technicus,* denotes those sensations that are produced by the stimulation of the specific pain end-organs, and which sensations may run through the whole gamut of intensity, from the very weakest to the strongest, and whose feeling-tone may vary from very pleasant, as in the gentle pricking of champagne, to most unpleasant, as in the torture of a renal colic.

The elementary sensation which can be analyzed out of the gustatory sensation-complex and is common to those of sparkling beverages, mustard, pepper, ginger, etc. is a sensation of pain, the feeling-tone of which is decidedly pleasant to most. In the "Stichempfindungen" produced by stimulating a pain-spot with a v. Frey's hair we have still pain of greater intensity than that in the compound sensation due to champagne or pickles. Here the sensation, if the hair be not too stiff, is generally without any particular feeling-tone. We have still pain, which now coincides with the physical or bodily pain of ordinary language, when we suffer from a severe toothache or an attack of gall-stone or gout, and where the feeling-tone is now highly unpleasant.

We do not, of course, assert that *all* the free-ending fibrils are pain end-organs. There may be end-organs of common sensibility which are of a similar or identical structure, but these no more give rise to sensations of pain than the end-organs for touch, warmth, or cold.

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[1] Article on Schmerzempfindungen in Nagel's *Handbuch der Physiologie des Menschen.* Bd III. p. 695.

We may now shortly refer to some other theories respecting the conduction of pain-stimuli. There is first the most widely adopted and popular theory that excessive excitation of any end-organ will give rise to pain. Thus a dazzling light gives rise to "Blendungsschmerz," a very loud sound, *e.g.* that of an explosion, invests the auditory sensation with painfulness, excessive stimulation of cutaneous touch-end-organs gives rise to cutaneous pain, etc. This theory will not stand investigation. There is no pain experienced with olfactory or gustatory stimuli, *qua* olfactory or gustatory. The pain experienced with a loud sound has nothing to do with the auditory sensation itself, but is due to the violent mechanical stimulus, the excessive stretching of the membrana tympani. The "Blendungsschmerz" is not due to excessive stimulation of the optic nerve but, as some think, to physiological changes in the choroidea or, more probably, to the excitation of the ciliary nerves by a violent contraction of the iris, since by paralysing them with homatropine this pain may be abolished. Wertheimer[1] has demonstrated by his experiments on dogs that the pain experienced on section of the optic nerve is not due to the fibres of that nerve but to pain nerves situate within, and in close proximity to, the optic nerve. Krause's[2] observations after the extirpation of the ganglion Gasseri point in the same direction. He found that patients could on the operated side look into the sun without experiencing any pain. Besides Feilchenfeld[3] admits: "He who expects a sensation as if his eyes were touched, pricked, cut, or pressed will not admit a Blendungsschmerz." This is tantamount to a confession that we are dealing with a feeling of unpleasantness, and not with a sensation of pain.

Another theory is that held by Goldscheider[4], namely that sight, hearing, taste, smell, heat and cold do not give rise to pain sensations, but that the sense-organs of touch do so; but as Behan[5] points out, touch and pain cannot have evolved the one from the other, because their distribution-areas are not the same. Pressure on the ulnar nerve makes pain and temperature sensations disappear, but not touch, localization or the muscular sense.

[1] "La douleur et les nerfs dolorifiques," *Année psych.* 1907, t. XIII. pp. 370–99.

[2] *Die Neuralgie des Trigeminus*, Leipzig, 1896.

[3] "Über den Blendungsschmerz," *Zeitschr. f. Sinnesphysiologie*, 1907, Bd XLII. pp. 313–48.

[4] *Über den Schmerz*, 1894.

[5] *Pain*, 1914, p. 32.

Oppenheimer's[1] view, that the vasomotor nerves transmit pain-sensations, is evidently wrong, since on section of the posterior roots analgesia is produced, whilst the vasomotor nerves continue to function.

There remains to be mentioned Wundt's theory, which, as far as the conduction in the spinal cord is concerned, is advocated in a modified form also by Goldscheider and others. Wundt holds that the end-organs of any sense may give rise to pain. But whereas the sensation proper is transmitted by the white fibres, pain travels to the brain through the grey matter. The criticisms advanced above apply also here.

We have up to the present, except on pp. 458–9 where we discussed Rivers' theory, been concerned mainly with cutaneous pain. Pain may, however, be elicited from the muscles and tendons and from the viscera. With the former strong pressure is the general *adequate stimulus*, whilst the viscera may be pinched, cut, pricked, etc., without producing any sensations of pain. These mechanical stimuli are not *adequate stimuli*. The adequate stimulus for most viscera is stretching or distension. Sherrington[2] says this about it: "The stimulation which excites pain in these internal organs is usually of mechanical kind, *e.g.* calculus, and the surgeon's knife and needle provide mechanical stimuli, and Haller and his co-workers in their research employed multiform stimuli, many of them mechanical in quality. But though mechanical, the latter are remote in quality from the former; *the former are distensile*. The action of a calculus can be imitated by injecting fluid of itself innocuous. Marked reflex effects can be excited from the very organs, the cutting and wounding of which remains without effect."

We have endeavoured to show in this section (1) that *Pain, in the proper psychological sense* of the term, must not be confounded with what is called pain in ordinary parlance and which is distinguished as "physical or bodily pain" and "mental or moral pain." The former, *i.e.* physical or bodily pain, is Pain in the proper psychological sense of great intensity and highly unpleasant feeling-tone, whilst the latter, *i.e.* mental or moral pain, has nothing whatever to do with pain, being merely the unpleasant affective content of a mental experience. (2) That Pain is a sensation of a definite modality of its own. (3) That its affective aspect may be pleasant, or neutral, *i.e.* absent, or unpleasant.

[1] *Schmerz und Temperaturempfindungen*, 1893.

[2] *The Integrative Action of the Nervous System*, London. 1909, p. 12.

III.

In the first section of this paper we have shown that from certain generally accepted premises there follows the logical conclusion that the affective side of our consciousness corresponds to definite neurone-processes, in other words, that there must be a centre for the feelings, pleasant-unpleasantness. It may be here in place to say a word about the term "cerebral centre" to avoid any misunderstanding. By a centre we do not necessarily mean a group of neurones situated in some particular spot or area of the brain, any more than that psychologists, who are a group of persons engaged in the elucidation of phenomena psychical, need, of necessity, all reside at the Psychological Department of University College. Some may live in different parts of London, others have their home in Oxford, Cambridge, Berlin or Leipzig, etc.; still they form a group bound together by community of interest, the individuals of which are in touch with each other and influence one another by their technical literature or by correspondence, etc.

In the second part we diverged and enlarged upon the nature of pain, showing that pain in the proper technical sense was a sensation which may or may not have a pleasant or an unpleasant feeling-tone. We considered that examination all the more necessary, since, as we shall see, the confusion of the sensation of pain and the feelings of pleasantness-unpleasantness is as great among neurologists as it is among psychologists. But whilst with psychologists the harm does not extend much beyond the misconception of the nature of pain, with neurologists the harmful influence may just begin to make itself felt at this point. For unless the neurologist is clear on the fundamental difference between the sensation of pain and the feelings of pleasantness-unpleasantness, his researches in this field may miscarry or their issue be obscured. The demonstration of a pain-centre is not *ipso facto* the discovery of a feeling-centre, and *vice versa*.

We shall not enter into a general discussion of the hypotheses that postulate a cerebral centre in order to explain the phenomena of pain or of unpleasantness—these are generally not distinguished one from another—nor do we intend to refute the theories of those investigators, like Meynert and others, who assume some general cerebral condition, *e.g.* anaemia and hyperaemia, as the physical basis of the feelings. What we propose is to bring forward certain further facts which bear on the conclusion arrived at above by deductive reasoning.

We may begin by citing a case quoted by Weir Mitchell[1]: "Dr Eve says he knew the patient; for years he was intimate with his family physicians. They had often spoken of Mr A's peculiar incapacity to feel pain. He became at last Dr Eve's patient and died of erysipelas, apparently made worse by repeated applications of tartar emetic ointment, used by the sick man in excess because they gave rise to no pain. The remainder I quote in full[2]: 'Mr A was about 56 years old at the time of his death. He was of sanguino-leuco-phlegmatic temperament; was a corpulent man, weighing about 250 lbs., and had been a free liver. He was a lawyer by profession, of good intellect, being a man of strong mind and body, and had acquired considerable reputation as an advocate and politician. And now, in relation to his possessing a natural state of anaesthesia, the following facts are submitted: During a political campaign, not liking the appearance of a finger injured in a rencontre, he bit it off himself and spat it upon the ground. He had at one time an ulcer on a toe, which resisted treatment for nearly three years. Mr A told his physician at the time, and has since repeated the same statement, that from first to last it never gave him any pain. An abscess also formed at one time in his hand, involving in its process the whole forearm and arm, which became enormously swollen up to the body and threatened his life. The lancet had repeatedly and freely to be used, and was followed up by a copious discharge of pus for several weeks. During the whole treatment he said he experienced no pain. He said he felt no pain when his eyes were operated upon for cataract; neither did they inflame. I can vouch for his statue-like immobility during the second operation. When his neck was pustulated by tartar emetic ointment he did not feel it, but ordered the application to be repeated. I made three incisions with a bistoury in his neck to relieve erysipelatous inflammation. He was so unconscious of the operation that after it was performed he asked me to do it, that he might turn over on his back in the bed. He told his attending physician that he never suffered pain from any cause whatever until his last illness. For some days after its development he complained of the erysipelas, and then passed into his usual insensible condition before the state of coma supervened. It is proper to say that Mr A was a man of great probity, and never boasted of being insensible to pain. The only cause suggested for this truly singular and peculiar condition of the system of this patient, is the free use of alcoholic potations, to which he was at one time much addicted. But others have drunk more than ever he did without producing the same result. We think the case of sufficient interest to deserve a passing notice.'"

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[1] "A Case of Natural Anaesthesia," contained in an article entitled "Precision in the Treatment of Chronic Diseases," *Medical Record*, XLII. New York, Dec. 24, 1892, 721–26, from Paul Eve's *Surgical Cases*, p. 771.

[2] *Southern Medical and Surgical Journal*, 1849, v.

This case is certainly of sufficient interest to deserve much more than "passing notice," which the author modestly claims. It is comprehensible if we look upon it as an instance of *psychical analgesia*, on a par with psychical blindness or psychical deafness. From the description of the case it is evident that the patient's psychical life was not devoid of feelings, pleasantness-unpleasantness, for no mention is made that these were absent, which would have been still more remarkable than the constitutional analgesia. On the contrary, we are told that he was of a "sanguino-leuco-phlegmatic temperament." Further, since he bit off his finger, it must have annoyed him, thus indicating an unpleasant feeling-tone, and the fact that he was "at one time addicted to the free use of alcoholic potations" reveals that they must have afforded him pleasure. Thus the case becomes important in a second point, inasmuch as it confirms our contention of the difference of the sensation of pain and the feelings, pleasantness-unpleasantness[1].

In turning next to give some account of the views, theories and speculations respecting a centre for Feeling we must bear in mind that we cannot separate this question altogether from that of a centre for pain owing to the confusion between the two. The account lays no claim to completeness; in fact, from the very nature of the case, it cannot be but fragmentary.

We may, in passing, mention Wundt[2] as a representative of the large class of authors who definitely deny the existence or necessity for the existence of a pain-centre: "But the more such phenomena depend upon the general properties of the central substance, the less do they justify the hypothesis, which has crept up now and again, of a specific pain-centre, or even the supposition that there exist for the pain-stimuli proper, paths that are completely separated from the other sensory nerves...All those properties of pain-excitation, in fact, remind one immediately of the excitability-conditions of the central substance."

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[1] Another most interesting case of entire absence of the Pain-Sense is reported by Erwin Stransky: "Zur Pathologie des Schmerzsinnes," *Monatschrift f. Psych. u. Neurologie*, XII. pp. 531-535, 1902.—Ref. *Zeitschrift f Psychologie*, XXXV. p. 71. F. Kiesow refers to a third case, *vide Archiv f. d. ges Psych.*, 1910, Bd XVIII, pp. 265-304

[2] *Grundzuge der physiol. Psychologie*, 5te Aufl. 1902, Bd II. p. 45.

Ziehen[1] affirms that "the feeling tones and affections, from the simplest to the most complicated ones, are psychophysical processes, *i.e.*......like all other psychical states they depend on the cortical parallel-processes. Decisive in this are especially our clinical observations in those mental diseases which, as we know from autopsy, are caused by a progressive destruction of the cortical elements, *i.e.* especially in Dementia paralytica and Dementia senilis. Without exception the destruction of the cortex goes hand in hand with an impoverishment of the affective life which leads ultimately to total apathy, to entire loss of all feeling-tones and affections." He then formulates his theory as follows:

"Positive affections increase the readiness for discharge, negative affections diminish it. Excitability, Excitation, and Dischargeability must be sharply distinguished one from another. The affections have not to do with excitability, but with the excitation itself and its dischargeability...The feeling component of the psycho-physical processes is identical with the readiness for discharge of the cortical cells. A certain sensational or ideational content corresponds to a certain process of change (*e.g.* a chemical change) in the cortical cells. In a certain change-process of this kind the readiness for discharge may be of varying degree, *i.e.* the tendency and ability for the propagation of the excitation (*e.g.* of the chemical change) into the association and projection-fibres arising from the neurone may be greater or smaller. The positive feeling-processes correspond to a greater readiness for discharge, the negative feeling-processes to a smaller readiness......From the standpoint of the discharge theory there may be the temptation to think also of an anatomical localisation. It has already been shown that the search in the cortex for a centre for feeling is in vain. One may, however, think of an intra-cellular localisation, and be inclined to ascribe, also in the sense of the discharge theory, to that part of the ganglion-cell to which falls the discharge of the excitation, viz. the so-called 'cone of origin' of the axis-cylinder process, a special relation to the feeling-processes."

Richet[2] denies the existence of special pain-nerves and, in order to explain the phenomena of *pain* (which for him is intense unpleasantness), assumes a pain-centre, the seat of which, he admits, is unknown, but which ends, he holds with Charcot and Türck, in the fibres of the posterior part of the internal capsule. So if there is a lesion in this part the peripheral excitations are no longer conducted. These fibres conduct at the same time sensory impressions, tactile, thermal, muscular, and impressions of pain. Higher up there would be distinct centres for each special sense. The pain-centre would be placed deep down and, owing to its anatomical and to its special dispositions, would offer a great resistance and would only function in reply to very strong stimuli.

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[1] "Physiologische Psychologie der Gefühle und Affekte," *Verhandlungen der Gesellschaft deutscher Naturforscher und Ärzte*, 75te Vers. 1903, Teil 1, pp. 44–61.

[2] "La douleur," *Revue philosophique*, 1877, t. iv, and "Les nerfs de la douleur," *ib.* 1896, t. vi. p. 716.

Behan[1] advances an interesting hypothesis. He says: "The small pyramidal cell-layer may also be concerned indirectly in pain production, since these cells are atrophied in Dementia, and may therefore be indirectly associated with sensibility; because it has been found that sensibility varies directly in proportion to the mental development of an individual, and that the pyramidal cells vary directly in proportion to the mentality."

Vulpian[2], basing himself on experiments of doubtful interpretation, localizes the *emotions* exclusively in the pons varolii (protubérance annulaire).

Sergi[3], like Todd, Hack Tuke, Laycock, Herbert Spencer, Brown-Sequard, Ribot, and others puts the seat of the affective phenomena in the bulb. What according to him speaks in favour of the importance of the bulb in the affective life is the number and nature of the nuclei situated between the pons varolii and the floor of the 4th ventricle, which act upon the heart, the blood vessels, the lungs, secretions, and intestinal movements. "The vital knot of Flourens is the vital centre and must also be the centre of pleasure and pain, which are only alterations of the functions of organic life." According to this view the rôle of the brain in the genesis of the affective states has been overrated. The brain acts only in two ways: (1) as means to render conscious all the troubles of organic life, which is the physical basis of the feelings; (2) as cause of excitations by means of ideas[4].

We may now examine some clinical observations which tend to ascribe the seat of the centre for the experience which is equivocally termed pain to the region of the thalamus. Greiff observed intense pains on the left side of a hemiplegic patient which persisted until death. At the autopsy he found two lesions in the right optic thalamus. Goldscheider[1], Eisenlohr[2], Bechterew, Biernacki, Henschen[3] and others had observed that certain lesions in the thalamus were painful. Edinger[4] reports a case of a woman where the pain was so severe that the patient committed suicide. In the autopsy it was found that the lesion in the brain occupied the dorsal part of the nucleus externus thalami and a part of the pulvinar and extended laterally from the pulvinar for 1 mm. into the internal capsule. Ferrucio Schupfer[5] is of opinion that pain has its origin in the posterior part of the pulvinar and that the lateral fibres of the internal capsule subserve the conduction of pain sensation. The labours of Dejerine and his school (Egger, Roussy, etc.) arranged into a complex, the 'syndrome thalamique,' the symptoms observable in lesions of the thalamus. Roussy[6] gives the following description of it. "The patients, generally without ictus, suffer from a slight passing motor hemiplegia which rapidly disappears, without epileptoid trepidation, mostly without the sign of Babinski. This hemiplegia is accompanied by subjective and objective disturbances of sensibility: subjective are the vivid and persistent pains on the paralysed side which yield to no treatment and which in themselves constitute a real infirmity (painful hemiplegia). Objective are the tactile, algesic and thermal hypoaesthesia or anaesthesia, with dysaesthesia, paraesthesia and topoaesthesia, sometimes hyperaesthesia. Still further persistent disturbances of deep sensibility, of loss of muscular sense, of astereognosis and hemiataxy. There are also frequently choreo-athetosi-form movements. Hemianopsy may be met with in lesions of the posterior and inferior part of the optic thalamus."

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[1] *Pain*, 1914, p. 11.

[2] "Leçons sur l'Anatomie du système nerveux," ch. xxiv.; quoted from Ribot, *Psychologie des sentiments*, Paris, 1908, p. 28. Also *Physiologie du système nerveux*, 1886, p 547.

[3] *Dolore e Piacere*, 1894.

[4] Quoted from Ribot, *loc. cit.* p. 28.

The most important work from our point of view, *i.e.* respecting a centre for the feelings, is that of Head and Holmes who were the first to point out the greatly exaggerated feeling-tone in certain cases of thalamic lesion.

The pains and uncomfortable paraesthesia, say these authors[7], have been explained by some observers as due to irritation[8]. This, these authors hold, cannot be the case. They say: "In the large majority of instances the lesion has proved to be a haemorrhage or a softening. Now all vascular lesions of the nervous system notoriously tend to produce the greatest disturbance of function at the time when they occur; the subsequent progress of the case always shows a certain amount of recovery. But in this group of thalamic cases the pains and over reaction come on, as a rule, during the stage of recovery of function, frequently at a considerable period after the 'stroke' has occurred, and they usually last unaltered for years."

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[1] *Berl. Klinische Wochenschrift*, 1894.

[2] *Arch. f. Psychiatrie und Nervenk.* IX.

[3] *Pathologie d. Gehirns*, 1903.

[4] "Giebt es einen central entstehenden Schmerz?" *Deutsche Zeitschr. f. Nervenheilkunde*, 1891, Bd I pp. 262–82.

[5] "Sui dolori di origine centrale," *Riv. speriment. di Fren.* 1898, XXIV. 582–604.

[6] *La couche optique*, 1907, p. 210.

[7] *Brain*, 1911, XXXIV. 102–254.

[8] *Vide* Roussy, *La couche optique*, p. 342.

"Moreover, we have pointed out that the response to pleasurable stimuli is also increased in some of the cases; a condition incompatible with the constant existence of an irritative lesion which provokes pain[1]."

As to the function of the thalamic region Head and Holmes state: "We know that paths run, not only from the optic thalamus to the cortex, but from the cortex to the thalamus, and they have been shown to terminate in its lateral zone, the very region affected in cases that have excited the '*syndrome thalamique.*'......The only function that can be ascribed to these cortico-thalamic paths is that through them the cortex controls, in some way, the activity of the thalamus...... Analysis of the over-response in these thalamic cases will, therefore, reveal what sensory qualities are due to the activity of the optic thalamus[2]."

"Afferent impulses on their way from the periphery to the cortex pay toll first of all to the unconscious co-ordinating mechanism of the spinal cord and cerebellum. Then, after being regrouped at the thalamic junction, they act upon two terminal centres. One of these, the essential organ of the optic thalamus, responds to all those elements which can evoke consciousness of an internal change in state, more particularly pleasure and discomfort. Sensory impulses then pass by way of the internal capsule to act upon the cortex, and these are the afferent materials out of which the cortex manufactures the forms of sensations with which it is concerned[3]."

These sensory impulses, they state, pass up in five main groups. The centre for the feelings lies then in the optic thalamus. "The primary centres of the cortex cannot receive those components which underlie feeling-tone; in this direction they are completely blocked...... In daily life all stimuli excite more or less both thalamic and cortical centres, for most unselected sensations contain both affective and discriminative elements[4]."

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[1] *Loc. cit.* p. 177. [2] *Loc. cit.* p. 179. [3] *Loc. cit.* p. 182. [4] *Loc. cit.* p. 190.

Sherrington[1] is of opinion that the pain-centre, as he calls it and which we should call the centre for unpleasantness, lies lower than the pleasure centre.

The symptom which then in this complex concerns us principally is the never missing "*Pain*" which yields to no treatment. We have, however, seen above, that the term *Pain* is used with a constantly varying connotation and we have, first of all, to be on our guard that in interpreting a clinician's account of a given case we do not read our own theories into it. We have to be careful whether the term "Pain" denotes the "*Sensation of Pain*" or the "Unpleasant Feeling." All the authors whom we have cited, until we come to Head and Holmes[2], mean, we believe, by the term "*pain,*" what in common parlance is meant by "*physical pain*" and which we describe as "*sensation of pain of unpleasant feeling-tone.*" A second, and to our mind a much greater, difficulty and danger is the clinician's interpretation of the patient's account of his experiences. Here we have not only to contend with the scrappy and superficial introspection of the untrained patient, but also with the loose terminology of both patient and clinician and the interpretation of the former's account of his experiences by the latter. Quite possibly it is owing to this difficulty that we have not met with any account of the exaggerated feeling-tone before Head and Holmes' paper mentioned above. Superficial observers may have easily interpreted as hyperaesthesia what, as Head and Holmes[3] point out, "...is susceptibility to the uncomfortable element of a stimulus..., rather than sensibility to pain. Not uncommonly the threshold may be actually raised to prick but lowered to pressure-pain, although both stimuli produce greatly increased discomfort over abnormal parts."

But it is not only the unpleasant feeling-tone which is increased, the pleasant feeling-tone too may become exaggerated. Besides, the sensation proper may be abolished whilst the feeling persists. "In a few cases when thermal sensibility was abolished," report the same authors[4], "warmth applied over a sufficiently large area evoked a feeling of pleasure. Thus, one of our patients found a hot water bottle pleasant and soothing to the affected foot, and did not recognize that it was warmth until he touched it with some normal part. In the same way many patients found the warm hand of the observer unusually pleasant on the abnormal side, although no such manifestations of pleasure were produced when it was applied to the normal parts of the body In one case, we were able to show that the patient could not recognize any thermal stimulus as such, and yet over the affected half of the chest large tubes containing water at from 38° C. to 48° C. evoked intense pleasure. This was shown not only by the expression of her face but by exclamations, 'Oh! that's lovely, it's so soothing, so very pleasant!' Temperature of 50° C. and above, or of 18° C. and below, caused great discomfort exactly as in most of these thalamic cases."

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[1] *Loc. cit.* pp. 254–5.

[2] "Sensory Disturbances from Cerebral Lesions," *Brain*, 1911, XXXIV. 102–254.

[3] *Loc. cit.* p. 130.

[4] *Loc. cit.* p. 134.

Later they state[1]: "The most remarkable feature in that group of thalamic cases with which we have dealt in this work is not loss of sensation, but an excessive response to affective stimuli. The positive effect (of excessive response to affective stimuli) as actual overloading of sensation with feeling-tone, was present in all our 24 cases of this class......The extent of the loss of sensation bears no relation to the amount of overaction to painful stimuli."

Head and Holmes[2] give also a highly interesting account of the behaviour of the affected half of the patient's body during certain states of emotion One of the patients was unable to go to his place of worship, because he "could not stand the hymns on the affected side, and his son noticed that during the singing his father constantly rubbed his affected hand." Of another patient it is said that "as soon as the choir began to sing, a horrid feeling came on in the affected side, and the leg was screwed up and started to shake." Still another patient stated that he had become more amorous since the attack: "I crave to place my right hand on the soft skin of a woman. It's my right hand that wants the consolation. I seem to crave for sympathy on my right side...My right side seems to be more artistic." And so on.

A somewhat paradoxical account is the following[3]: "When the observer gently scrapes with his finger the patient's palm, this under normal conditions is not unpleasant; but on the affected side the patient may cry out and attempt to withdraw his hand. His face is distorted with discomfort. One patient complained: 'It is a horrid sensation, as if my hands were covered with spikes and you were running them in; it is not painful but very unpleasant.'" We have then here a sensation which under normal condition is *not unpleasant* transformed into one of great unpleasantness; that is to say, it does not present the exaggeration found in all the other cases and required by the authors' theory.

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[1] *Loc. cit.* p. 177. [2] *Loc. cit.* pp. 135–6. [3] *Loc. cit.* p. 132.

It is a pity that Head and Holmes did not apply the same stimulus to the palm of the hand of the normal side, so as to make quite sure that the normal sensation in their subject was really "not unpleasant." This uncertainty caused the author to repeat the experiment with three of his trained subjects. The following protocols show how complex an experience is produced by the stroking of the palm of the hand, and how great the individual differences:

SUBJECT F.

A very complex experience. The content of consciousness is almost continuously varying and the feeling also varies frequently both in intensity and in quality. To begin with there were very distinct cold sensations which were slightly unpleasant. At some points there seemed also to be heat sensations which were slightly pleasant. The tactual sensations were sometimes indifferent, more often slightly unpleasant, sometimes quite markedly unpleasant. At these times there was an experience which could perhaps be best described metaphorically as going against the grain, or rubbing the wrong way. I could distinguish at these times a roughness and jerkiness in the tactual sensation and a widespread organic sensation which seemed to radiate from the hand. A tendency to shrink all through the body. An unpleasant sensation seemed to be localized in the mouth and especially in the upper teeth, particularly the last element (*i.e.* the sensation from the teeth) was very unpleasant. Then there were in the hand a number of rapid and vivid sensations which were like the experience of being tickled. I think I could discover in these temperature and pain components. They were localized at a number of discrete points and the sensations at each point appearing and disappearing. These sensations were very pleasant and during the moments of their appearance almost monopolized consciousness. Very little if any coexistence of feelings. The sum total of the experience is unpleasant. In the period immediately preceding the application of the stimulus, and perhaps during the first moments of its application, there was a slight emotion of repugnance dimly connected with the fact that the experimenter was of male sex.

SUBJECT J.

At first there was a sensation of contact which was neutral as regards feeling-tone. Then there was a little tickling sensation which had a slight pleasant feeling-tone in the hollow part of the hand. Towards the end the sensation was again neutral in feeling-tone. I was also conscious of the movement across my hand, *i.e.* the succession of points in the hand. The feeling-tones were localized. On the whole the experience was slightly pleasant.

SUBJECT P.

At first I could distinguish the sensation of pressure only. It was pleasant rather than unpleasant. Then I had distinct sensations of temperature, quite distinct from tactual sensations. The feeling-tone was pleasant, in fact slightly more pleasant when I became aware of the temperature sensations. Then the warmth-sensations were succeeded by quite unpleasant tickling sensations which remained after the stimulus was withdrawn. I don't think I can distinguish the feeling-tone from the sensation in this case.

Thus with many people at any rate the normal sensation *is* unpleasant. If this happens to have been the case with this particular patient of Head and Holmes then the symptom was not paradoxical but on the contrary perfectly fits their theory. On the other hand it may possibly be an illustration of the danger against which we have warned above, viz. a faulty description of the patient's introspection. As we have pointed out, when giving an account of our own experiments, reliable introspection for scientific purposes is difficult at any time and requires observers trained *ad hoc*. In the case of the feelings this is infinitely more difficult than in cases of cognition, and we judged it advisable to give our observers, although they were all without exception trained psychologists, a special training for the purpose of our experiments. We hope we shall therefore not be considered presumptive in offering here some criticisms and suggestions.

The examinations of the patient's sensations and feelings are of the utmost importance and may, if the results are unequivocal and reliable, lead to valuable conclusions. To obtain such results it is, first of all, necessary to have to deal with an intelligent patient whose mental disposition and general state allow him to receive a short but sufficient training in introspection, so as to render it reliable and of value for psychological purposes. The next important question concerns the experimenter. As few clinicians have received that training which is necessary for the conduction of such psychological investigation, the investigation ought to be entrusted, wherever the said difficulty exists, to an experienced experimental psychologist. But even where the clinician is an experimental psychologist himself and capable of undertaking the investigation, it may still be advisable to hand it over to a pure experimentalist as has been done with marked advantage by Head and Rivers[1]. For as Titchener[2] has pointed out, the danger, especially in the case of the feelings, of influencing unknowingly the results by the hidden bias of the experimenter is enormous. The psychologist can approach the case quite unbiased, ignorant about any diagnosis, so to speak objectively, much in the same way as the pathologist would examine a sample of blood or of spinal fluid for a Wassermann-reaction, or the bacteriologist a sputum for Koch's bacillus. It is, of course, essential that the patient be not spoiled before by unskilful preliminary experiments. How unscientific, from a psychological point of view, clinical examination frequently is, has been pointed out by Rivers and Head[1]. After the division of a branch of the nervus radialis in Head's arm cutaneous touch was in a certain area of the forearm and hand entirely destroyed, whilst deep sensibility remained intact. However by ordinary clinical examination, touching the area with pencil, the sense of touch was pronounced to be intact: "All those stimuli commonly used by the clinician to test the presence of 'touch' were appreciated and well localized. Mr Dean (*i.e.* the surgeon who performed the operation), who was not familiar with our previous observations, said that he should have thought that sensation of touch was intact, had he not known the nerves had been divided[2]."

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[1] *Vide* below.

[2] *Lectures on Elementary Psychology of Feeling and Attention*, 1908, p. 48

Another most important point is that a full protocol be kept and made available, of the preliminary and the actual experiments; for an opinion on or a criticism of the conclusions without full access to the whole of the experiments is quite futile in any investigation of the feelings, as pointed out by Titchener[3] in his criticism of the work of Johnston.

Further observations and investigations of cases, like those of Head and Holmes mentioned above (p. 471), with a full and detailed description of the experiments are sure to prove of the highest importance not only for neurological and physiological inferences, but also for psychological deductions.

In further observations on suitable patients suffering from thalamic lesions the following points may deserve careful attention. How do the patients enjoy their meals, do they chew food they like on the affected side, and food they dislike, or swallow nasty medicine, on the unaffected side? After proper training in introspection experiments in vision are desirable. Comparison of the two sides of the visual field and of the monocular fields of both eyes are worthy of investigation. Many similar experiments may be devised to investigate the dependence of the feelings upon the side of the lesion.

Besides these clinical observations there are also observations in numerous experiments on animals by several investigators. Into these, however, we have not entered here. Apart from the technique in producing these lesions which has been repeatedly and severely criticised, there are other grounds on which we consider any experiments on animals made for the purpose of elucidating the feelings or even pain quite valueless. The reason is that there is no objective reaction to indicate beyond doubt whether an animal experiences a pleasant or an unpleasant feeling or suffers pain. Richet[1] says: "Pain coincides with the stopping of the heart, the dilation of the iris, the lowering of the blood-pressure, but it is not pain that produces these reflex-acts. They are merely simultaneous and produced by the same cause." C. S. Myers also has pointed out when criticising Sherrington's experiments on dogs, performed to disprove the James-Lange theory of the emotions, that, until a dog can tell us what it is experiencing, we remain ignorant as to its feelings or sensations. The wagging of the tail or growling may in the cases under observation be mere reflexes without any of the feelings they normally indicate.

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[1] "A Human Experiment in Nerve Division," *Brain*, 1908, XXXI. 323 *seq.*

[2] *Loc. cit.* p. 326.

[3] *Loc. cit.* pp. 48–49.

IV.

In concluding this paper we will in general outlines recapitulate its contents.

In the first part we dealt with the

Logical Argument for a neural correlate or nerve-centre for t[illegible] feelings. Given as major premise the generally accepted proposition that every elementary state of consciousness corresponds to a definite nervous process, to the excitation of particular sets of neurones; and as minor premise the proposition which is usually accepted and which we endeavoured to corroborate, that the feelings are elementary states of consciousness; there follows the conclusion that the feelings correspond to definite nervous processes, to excitations of particular sets of neurones. In other words, if we accept the two propositions stated above we are bound to accept a centre for the feelings: Pleasantness-Unpleasantness. We then gave examples taken from some of the best known psychologists and eminent philosophers that this logical conclusion has been generally violated.

In the second part we enquired into the

Nature of Pain. We pointed out the great havoc that has been caused in psychological literature by accepting as a *terminus technicus* a word of a most indefinite and varying denotation, and we showed that pain was thus often regarded as a feeling, as the acme of unpleasantness. We endeavoured to prove that this was not so, that pain was a sensation of a definite modality the feeling-tone of which was generally, but not necessarily always, unpleasant, that it could only be produced by the excitation of specific end-organs, that violent excitation of other sense-organs gave rise to sensations of great intensity accompanied possibly by great unpleasantness, but not by pain, unless the stimulus also excited specific pain end-organs. This we considered necessary because the demonstration of a pain-centre would not *ipso facto* mean that a centre for the feelings had been established and *vice versa*.

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[1] *Revue philos.* 1877, tome IV. pp. 457–481.

In the third part we passed in review some of the theories of, and also the

Evidence for, a neural correlate of the feelings. We interested ourselves particularly in the syndrome thalamique and quoted freely from the evidence of Head and Holmes which tends to show that the centre for feeling has probably its seat in the lateral zone of the optic thalamus. We also pointed out that to obtain a greater number of reliable and uniform results from suitable thalamic cases it would be advisable to hand them over for careful investigation to the trained experimental psychologist. Animal experiments, it was shown, are quite useless [illegible]r an investigation of the feelings.

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FREUDIAN MECHANISMS AS FACTORS IN MORAL DEVELOPMENT.

By J. C. FLÜGEL.

I. *Introduction. The ethical significance of mental conflict, as revealed by Freud. The various possible solutions of mental conflict.*
II. *These solutions considered from the ethical point of view: repression.*
III. *Displacement, neurosis, sublimation.*
IV. *Conscious control. Its integrative function.*
V. *Various forms of solution of conflict under conscious control.*
VI. *The* rôle *of knowledge and of feeling in conscious control. Freud and Socrates.*
VII. *Repression, displacement and conscious control considered in the light of criteria of evolution.*
VIII. *The relation of conscious control to cultural development as manifested in institutions, beliefs, etc.*
IX. *Psychological limitations of the principle of conscious control. Conclusion.*

I.

The psychology of Sigmund Freud, with its patient tracking down of the unconscious bases of conscious thoughts and motives, its clear vision of the different strata of the mental life, and, above all, its penetrating illumination of the mental characteristics of the infant and of the infantile psychic residua of the adult, is probably destined to cast a greater light than any other contemporary system upon the problems connected with the development and evolution of the human mind. In this respect it seems not unlikely that the further advance of mental science will reveal Freud as the true successor of Darwin and Spencer, who were the first definitely to introduce the evolutionary point of view into Psychology. It is true that Freud in his own writings has not often been at pains to emphasise the evolutionary bearing of his doctrines; nor have his followers connected his work with that of the earlier evolutionary writers to the extent that might perhaps have been expected. It would seem probable however that when the flood of controversy which at present rages round Freud's work begins to abate, and when his discoveries begin to take their accepted place in the body of academic psychological doctrine, the true value of his work for the proper understanding of psychical and social development will become increasingly apparent. In the present paper an attempt will be made to deal specially with the ethical aspect of Freud's contributions to the study of the development of mind, and to draw from some of his conclusions a few inferences which would appear to be of interest and importance from the point of view of Applied Psychology and Ethics.

The existence of Ethics presupposes the possibility of two or more courses of action, one of which is morally preferable to the other. It presupposes further that these courses of action shall be somehow present as possibilities to the mind of the actor, that, in fact, the actor shall be confronted with conflicting tendencies to action. Now it is just with these conflicting tendencies to action that Freud has concerned himself more, perhaps, than any other psychological investigator. Most of the work that has been done in this field so far, has been concerned —on the one hand—with researches of a more or less definitely physiological nature, such as those on the interaction of reflexes, which, important as they are even from the purely psychological point of view, are yet too far removed from conscious choice to be of much interest to Ethics; or—at the extreme other end of the scale—with the process of deliberation, which is not at all typical of the way in which conflicts of opposing tendencies usually are settled in the human mind, and which (as has become increasingly recognised of recent years) when considered by itself tends to produce an exaggerated estimate of the part played by the rational and an undervaluation of that played by the affective and emotional factors involved in choice. The work of Freud and his school to some extent fills up this gap, by throwing light upon the nature of the unconscious and dimly conscious affective factors which are apt to play such an important part both in weighty and in trivial decisions. At the same time Freud's work by showing that all mental development in the individual human mind is, to a great extent, the result of a series of conflicts between opposing tendencies, has given an added interest and importance to the whole subject of mental conflicts and to the study of the various methods by which the mind is able to arrive at a solution of such conflicts. The increased knowledge which Freud has given us of the *nature* of the conflicting tendencies involved in moral conflicts, though on the whole of greater importance to Psychology than to Ethics, nevertheless opens up a number of fresh and hitherto largely unsuspected problems of the greatest interest even to the purely ethical enquirer[1].

In the present paper however, we shall be concerned more particularly with the *form* taken by the moral conflict, with the various possible solutions of the conflict and with the relative moral value of these solutions.

The various ways in which the mind endeavours to find a solution for the conflicting tendencies aroused within it, have been clearly formulated by Freud and his expositors. Although the precise classification adopted by various writers has differed somewhat in minor details, it is probably true that there would be general agreement in classifying the different solutions under three main heads as follows[2]:

(1) Repression, in which an endeavour is made to force one of the conflicting tendencies out of the mind altogether, to deny it any satisfaction whatsoever and, as far as possible, to ignore its existence.

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[1] Thus, the highly important part which Freud has shown to be played by erotic tendencies, emphasises the importance of all questions concerned with the ethical principles of sexual instruction and hygiene and of the general attitude towards questions of sex; the transition from auto-erotism to hetero-erotism ("object love"), to which Freud has drawn attention, will almost certainly be found to have immensely important bearings on the ethical problems connected with egoism and altruism (cf. Freud, "Zur Einfuhrung des Narcissmus," *Jahrbuch der Psychoanalyse*, VI. 1); the discovery of the parental complexes and the other important affective tendencies connected with family relationships call for a revision of the Ethics of family life in the light of our new knowledge; while the whole doctrine of Displacement and Sublimation has revealed the intimate interdependence of 'lower' and 'higher' activities, and must surely affect our attitude towards the former, as being only more primitive manifestations of the same energy that, at a higher stage of development, gives rise to the latter.

[2] As is usually the case with psychological distinctions of this sort the three kinds of processes here indicated must not be regarded as of necessity mutually exclusive in any given conflict. Thus some degree of Repression would seem necessary for Displacement, while Repression of certain elements of a conative tendency might very well coexist with the Displacement of other elements, which were less in conflict with other simultaneous tendencies. There may also, of course, be quantitative differences within each of the three divisions here given. Thus Repression may be more or less complete; the whole or only a part of the energy inherent in the original tendency may be displaced or sublimated; the conscious deliberation on the factors involved may be more or less exhaustive and unprejudiced.

(2) Displacement, in which one of the conflicting tendencies, while not allowed to work itself out in its original form, nevertheless succeeds in finding expression in a modified, often roundabout, way; a way in which it does not conflict to the same extent with the other tendency or tendencies to which it is opposed. When this modified expression of the tendency is of socially or morally superior value to the original expression, the process of modification or Displacement is spoken of as Sublimation.

(3) A process of deliberate choice, preceded by an unprejudiced enquiry into the nature of the conflicting tendencies and the probable consequences of the actions to which they impel. We shall in future refer to this process under the name of Conscious Control, a term which, though possibly open to objections in some respects, has nevertheless the merit from our point of view of clearly distinguishing the essential character of this process from that of the other two.

We will now proceed to investigate in turn each of these three methods of solving conflict, with a view to determining as far as possible the ethical value of each method of solution and its place in the scale of mental and moral development.

II.

As regards Repression, there can be no doubt that the process occurs in many different degrees of intensity and extent; there are also great variations as regards the mental level at which it takes place. At one end of the scale it may appear as a fully conscious event, as when we definitely abandon certain hopes, give up the indulgence of certain tendencies, or determine to put certain thoughts out of our mind. At the other extreme, the repressed tendency may never appear in consciousness at all (except perhaps as the result of psychoanalytic procedure) and its existence can normally only be inferred by observing indirectly its effects on thought or action. As to the nature of the tendencies which most frequently undergo Repression, it is well known that the observations of Freud and others (obtained by psychoanalytic work on pathological cases) have shown the content of the repressed thoughts and tendencies to be predominantly sexual in character, or else of a markedly aggressive or anti-social type (though here also a sexual element is frequently not wanting). In the cases which have hitherto most often been made the subjects of investigation, it is thus the tendencies which are most strongly tabooed by moral or social precepts and conventions, which most commonly suffer Repression. It is probable, however, that in another class which have less frequently been subjected to thorough analysis, it is rather the tendencies to act *in accordance with* traditional morality that have been repressed, and the opposing tendencies of a relatively anti-social or immoral character that have been allowed to triumph.

In forming a judgment as to the ethical value of Repression and its place in the scale of moral development, it is necessary to have clearly in view the manner in which the process of Repression affects the thoughts, feelings and actions of the person it concerns. In so far as Repression is complete, it represents a clear victory of one tendency over another to which it is opposed, so that thought and conduct proceed entirely in accordance with the dictates of the former tendency and are unmodified by those of the latter. At the first glance, it is apt to appear that, provided it is the anti-social and immoral tendencies of human nature that are subject to Repression, this state of affairs is in every way an eminently desirable one. It is in fact one that is aimed at by many of the more crude and primitive forms of moral instruction and endeavour But further consideration of the true nature of the process concerned, as far as the present state of psychological science enables us to understand it, serves to bring to light certain grave disadvantages attendant upon the successful repression even of undoubtedly harmful and anti-social tendencies.

In the first place the energy inherent in the repressed tendencies is definitely dissociated from the rest of the personality, and, in so far as the repression is entirely successful, this energy is prevented from finding a fresh outlet, and thus eventually becoming available for some useful purpose, as in the case of Sublimation. Indeed it would appear that in cases of severe Repression, the original tendencies are apt to persist in an almost entirely unmodified form, so that the person who is the subject of such Repression perpetually carries about with him immoral motives of a crude and primitive type, which constitute a serious danger to his moral well-being; for they are always ready to burst forth from their prison-house and find expression in thought and action, should the customary Repression be for any reason relaxed[1].

Secondly, the enforcement of a successful Repression such as we are here contemplating, demands the exercise of considerable energy on the part of the repressing force; energy, which might otherwise be available for more positive and useful ends. This is seen clearly in those cases of Neurasthenia, where, owing to a great part of the total psychic energy being occupied in a subconscious conflict between the repressing and repressed forces, a relatively small quantity of this energy only is available for the usual business of life, and undue fatigue and lassitude are consequently induced by a comparatively small amount of labour. In these cases the solution of the conflict and the consequent setting free of the opposing forces result in an increased supply of energy for ordinary purposes and much greater freedom from fatigue.

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[1] The unprovoked atrocities of which the armies, even of civilised nations, may be guilty in time of war, are probably to a great extent to be regarded as manifestations of repressed tendencies consequent upon the diminution or removal of normal inhibitions.

Thirdly, in connection also with the enforcement of Repression, there would seem to be invariably present certain more specific disabilities, which may on occasion take the form of emotional or cognitive defects of very appreciable gravity. These specific effects of Repression result from the necessity of guarding against the arousal of the repressed tendencies by means of associative connection. There would seem to be in the main two ways of meeting this necessity, though they are far from being mutually exclusive. The first consists in depriving an event of all feeling or emotional value, in cases where the feeling likely to be called up is one allied or similar to that of the repressed tendency; or else in providing against the arousal of such feeling by substituting a feeling of an opposite, or at least a different, quality. In actual life this process gives rise to various forms of prejudice and bias, and causes inability to react normally to, or judge soundly of, certain situations, problems or events. The typical forms of such reactions are to be observed in matters relating to sex, the admittedly important problems of which few people are able to deal with normally, or even to discuss intelligently, because of repressions which give rise to embarrassment or even to disgust: there are, however, almost certainly, in the lives of all of us a number of problems of many different kinds, in which our reactions are impeded or misguided by emotional disabilities of this nature.

Theoretically distinguishable from this emotional distortion induced by Repression, but in practice often closely connected with it, there are found disturbances of a more purely cognitive nature, which are at least equally subversive of normal adaptation to the problems presented by our environment. Under this head come many of those cases of forgetting, mistaking, misplacing, etc., which have been studied by Freud in his *Psycho-pathology of Everyday Life*. It is probable that many of us are prevented from finding the correct solution to quite simple problems by mechanisms of this kind, and it is obvious that a number of severe Repressions might thus easily give rise to disabilities of quite a serious nature.

We have hitherto been assuming that the Repression is completely successful in preventing the repressed tendency from exerting any positive and *direct* influence on thought or action. It must be recognised however that this is an extreme case and that in the majority of instances a repressed tendency does manifest itself positively to some extent and in some manner[1]. Probably the most frequent way in which the repressed tendency finds expression is by means of the mechanism of Displacement, which we shall proceed in the next section to consider more in detail. In other cases however the tendency may in part escape from the Repression more or less in its original form, and in these instances the opposing tendencies alternately inhibit one another. In a struggle of this nature one tendency is usually on the whole stronger than the other, and in this case the weaker, repressed, tendency can only break through occasionally for short (often momentary) periods, and usually manifests itself in 'everyday' pathological symptoms not unlike those to which we have just referred, except that here the pathological effects are due to an uprush of the repressed energy from the Unconscious and not to the influence of the repressing force. In other cases there may be a conflict in which the combatants are more or less equally matched, and in which sometimes one, sometimes the other, has the upper hand, but in which neither has the whole force of the psychic energy behind it.

Both the advantages and disadvantages of such a condition as compared with one of more complete Repression are fairly obvious. On the one hand there is greater freedom from prejudice and from the more specific disabilities imposed by the repressing force, together with greater opportunities for solution of the conflict by way of Sublimation or Conscious Control. On the other hand, there is more danger of the overwhelming of conscious thoughts and tendencies by a sudden uprush from the Unconscious, greater liability to the undesirable and pathological forms of Displacement and (especially in the case of conflicts between tendencies of more or less equal strength) a want of co-ordination and a certain degree of incoherence in the sequence of thoughts and actions, due to the fact that no one tendency is able to manifest itself uninterruptedly for any considerable time. The man who has his repressed tendencies well under control will live a more consistent and even-tenored life, though, as we have seen, he will suffer from certain limitations necessarily consequent upon the very success and completeness of this control. The man whose repressions are less thorough will tend to lead a more erratic and irresponsible, though possibly more brilliant and more many-sided, life, with possibilities of wider sympathy and understanding, but with greater liability also to moral laxity or to certain forms of mental disorder[1].

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[1] Indeed it is quite probable that what has been called above "entirely successful" Repression never actually occurs and that there is always to be found *some* positive manifestation of the repressed tendencies, direct or indirect. In the present connection, however, the conception of such an entirely successful Repression (i.e. one which does not permit any positive manifestation of the repressed tendency and which can only be detected, if at all, by the diminution of generally available energy caused by the conflict involved in the Repression and by the various specific disabilities we have just been considering) would seem to be justified in that it represents the process under consideration in its purest form—a form to which the more complete instances of the process constantly tend to approximate.

III.

As has been already remarked, Displacement is the method most usually employed in dealing with incompletely repressed tendencies. It consists essentially in the outflow of the repressed energy through some other channel than its original or most natural one; through which latter it was prevented from escaping by the inhibiting influence of some opposing tendency: the new channel being of course relatively or entirely free from such influence. The process has to some extent been long familiar to psychologists (as also, of course, to physiologists). Indeed the parallelism with the similar manifestations of physical force is sufficiently striking to intrude itself upon them as soon as they began to operate with the concept of 'nervous' or of 'mental energy.' But Freud and his followers were the first to investigate at all thoroughly the details of the process. As a result of their study it has been shown that the actual form of the Displacement represents, as it were, a compromise between the conflicting tendencies. It retains some associative connection with the original expression of the repressed tendency, but at the same time takes such a shape as to satisfy also the opposed repressing tendency. It is in this connection that Freud speaks of the repressing force as playing the part of a 'Censor,' which veils and modifies the expression of the repressed tendency until it no longer appears to offend against the dictates of morality and may therefore be allowed to pass. From this action of the Censor arise the greater part of the distortions and symbolisms which it is the business of Psycho-analysis to interpret, and the general principles of which are familiar to all who have studied the literature of that method of psychological inquiry.

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[1] It is perhaps interesting to note that the results of greater or less Repression as here indicated bear considerable resemblance to the 'W' and 'Non-W' types recently shown to exist by Dr E. Webb (*Brit. J. of Psychol., Monogr. Suppl.*, No. 3). The 'W' type seems on the whole to correspond to persons with repressed tendencies well under control, though it might also include persons whose repressions have been successfully released by Sublimation, or whose actions are guided to a large extent by Conscious Control. On the other hand, it seems likely that incomplete repressions and the relative incoordination which they tend to bring about play an important part in the production of the principal characteristics of the opposite 'Non-W,' type.

We are here concerned with the ethical aspect of the process, and on turning to the problems connected with this, it is obvious, from even a slight acquaintance with the facts, that the moral value of the Displacement varies enormously in different cases. On the one hand, there is evidence that tendencies which would be harmful, or at best undesirable, if allowed to find expression in their original form, may be transformed into activities of a highly useful and socially beneficial character. In cases like these, as already indicated, the process of Displacement has been given the name of Sublimation. As examples may be mentioned such transformations as are effected in a man who, originally endowed with impulses prompting to cruelty and sadism, becomes an efficient surgeon, soldier or butcher; or in one in whom an interest and proficiency in sculpture can be traced back to an original impulse to play with faecal matter; or in another in whom an original crude tendency to exhibitionism has found expression in the pulpit, on the platform or on the stage. These are perhaps extreme cases, but instances of the kind are probably much more frequent than is often suspected[1]. Indeed, even before the work of Freud had demonstrated more precisely the mechanisms involved, and studied in detail the exact nature of the transitions, it had become generally recognised that much of the energy displayed in artistic, religious or even scientific and philosophic activities, had sprung originally from cruder and more primitive tendencies, often of a sexual nature.

It is obvious, indeed, that this process of Sublimation constitutes one of the most potent mechanisms of mental development, both in the individual and in the race. It is manifestly a great advance upon mere Repression, inasmuch as it sets free for positive and beneficial ends the energy that would otherwise be uselessly and wastefully penned up. It is true that Sublimation depends upon the same mechanism as Repression in so far as it presupposes a conflict of opposing tendencies, a conflict, however, which, in the case of Sublimation, is settled by the satisfactory reconciliation of the opposing forces, instead of a mere overcoming and imprisoning of one force by the other. As the result of Sublimation we attain a relatively smooth and frictionless working of the whole psychic force concerned towards the production of an external result, whereas in Repression, at best only a portion of this force is at disposal for this purpose, and in most cases the working of the mind is impeded by constant friction and struggle, leading to inco-ordination of behaviour and inability to pursue one end consistently and uninterruptedly.

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[1] Cf. Stekel, "Berufswahl und Neurose," *Arch. f. Kriminal-Anthrop. etc* vol. XLI. 1911; Ernest Jones, "The Value of Sublimating Processes for Education and Re-education," *Papers on Psycho-analysis*, 1913.

In view of this superiority of Sublimation over Repression it is evident that further understanding of the precise mechanisms involved in Sublimation and of the methods of inducing it is likely to be of the greatest service to Applied Psychology, both in its therapeutic and paedagogic branches[1]. Unfortunately perhaps from this point of view, the actual associative connection in virtue of which the passage of energy into the new channel is made possible appears to take place usually, if not indeed always, in the Unconscious. It is neither essential nor sufficient that there should be conscious recognition of the similarity between the original and the sublimated expressions of the tendency in question. Indeed, these similarities are often of the peculiarly illogical and fantastic kind which is characteristic of the Unconscious, but which is apt to appear far-fetched or ridiculous, and thus to make little appeal to Consciousness. For this reason it would appear to be difficult or impossible to make Sublimation a matter of instruction or education in the ordinary sense. The most that we can do is to encourage it indirectly, by providing opportunities for the outflow of partially repressed tendencies into beneficial, or at least harmless, channels; trusting that under these circumstances the necessary associations for the successful carrying out of the desired Sublimations will be formed subconsciously. It is possible that in the not too remote future we may be guided in this work by the gradual accumulation of psycho-analytic knowledge as to the actual forms most frequently taken by Sublimations; such knowledge enabling us to provide the most suitable opportunities for each kind of tendency, as regards which Sublimation is desired.

We must remember moreover that the more advanced forms of

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[1] Cf. Ernest Jones, *op. cit.*

Sublimation are in most cases processes of gradual growth through successive steps, each more remote in nature from the original expression of the tendency, and each representing an advance from the moral point of view. Thus we must not grow impatient if in the first stages there is to our eyes still too much resemblance to the original activity, *e.g.* if, in the case of the sculptor we were considering above, the boy began his career with a passionate delight in the formation of mud pies, or if the subsequent surgeon early displayed an undue tendency to pull to pieces flowers, toys or other objects or to behave in an aggressive and cruel manner in his relations with other children. The gulf between the original tendencies and their ultimate expression is obviously far too wide to be bridged by a single step, especially when we are dealing with minds that are still immature. For this reason it would seem very undesirable in educative practice to extend our taboos and prohibitions beyond the sphere in which they are strictly necessary, lest in so doing we interfere with the initial stages of Sublimation. A school or home (or even a country) in which everything is forbidden will probably afford a relatively unfavourable atmosphere for the growth of Sublimation.

In confining our attention hitherto to those cases of Displacement which come under the head of Sublimation, we have been considering exclusively those beneficial forms of Displacement which can receive more or less unqualified approval from the moral point of view. But, as already indicated, not all Displacements are of this desirable character. There are many cases where the advantage of the modified expression of a tendency over its original expression is slight or doubtful, as where, for instance, repressed sexual desires find an outlet in religious fanaticism, or where a tendency to exhibitionism manifests itself in an undue preoccupation with the ornamental aspect of clothing, or a tendency to coprophilia in an absorbing passion for the hoarding of wealth.

In still other, unfortunately very numerous, cases the Displacement assumes a definitely pathological form and gives rise to a phobia, tic, obsession or one of the many other manifestations of neurosis with which current psycho-therapeutic literature (and indeed the experience of everyday life) makes us so familiar. The extreme prevalence of these forms of Displacement and the enormous increase of misery and decrease of human efficiency for which they are responsible should make the study of their causes, treatment and prevention one of the most pressing problems both of medicine and of educational hygiene. As regards their treatment and the understanding of their causes, much has been done by modern psycho-therapeutic methods and our knowledge in this direction is still rapidly increasing. Less perhaps has been accomplished as regards prevention, though to some extent the study of the causes and of the means of treating these cases of Displacement has, as was to be expected, thrown considerable light also on the question of the most suitable measures of prophylaxis. The necessary knowledge has not yet been disseminated among those by whom these measures will most usually have to be applied, such as parents, teachers and others who have charge of the young and of the adolescent: nor have there indeed been made those detailed applications of such general principles as we possess to the individual problems of training and upbringing which will probably be necessary before we can reap the full fruit even of our present knowledge. There are still, moreover, some more general questions which would seem to require much further elucidation in this connection. In particular, it would seem very important to understand the precise conditions which determine where a Displacement shall take a beneficial form (Sublimation) or a pathological one (Neurosis). The opinion of most psycho-analysts would seem to be that the capacity for Sublimation is limited (though of course there are considerable individual differences in this respect) and that if an attempt is made to repress too rapidly, too completely or too extensively, then repressed tendencies are liable to find an outlet through harmful rather than through beneficial channels, because all the latter that are available have already been used up. If this view is correct it would appear that Repression is frequently carried out beyond the limits of what is practically useful or ethically desirable; that there has in fact arisen at the present stage of our moral development a "disharmony" in this respect, which, like most other "disharmonies," is productive of much suffering to the individual and is a serious hindrance to the progress of the race. In what way, if any, is this "disharmony" destined to be overcome at the next stage of our development? It would seem that the most likely answer to the question is afforded by a consideration of the third of the above mentioned methods of solving Conflict, to which consideration we will now proceed.

IV.

The methods of solving conflicts between opposing tendencies with which we have hitherto been concerned have but little connection with consciousness and practically none at all with the process of deliberate choice. The ability to look facts in the face, to reason impartially as to their probable consequences and, above all, to realise courageously and without prejudice our own feelings and tendencies towards them, is conspicuously lacking in every process where Repression plays a part. Where this mechanism is acting strongly, we receive a distorted impression of our environment and a still less adequate presentation of ourselves; the grounds on which decisions are made are very far from being based on that calm, unbiassed scrutiny of all the facts concerned, upon which the judgments necessary for a scientific control of conduct should be framed. Nor do the thoughts and feelings of one who is the subject of considerable repressions show the inner consistency which is characteristic of the scientific point of view. On the contrary, the mind of such a person exhibits a marked degree of dissociation, the different thoughts and tendencies being shut off from one another in "logic tight" compartments and being thus unable to interact or to influence one another in a normal way.

The moral, intellectual and therapeutic value of Freud's method of Psycho-analysis would seem alike ultimately to lie in the fact that it affords a means of leading the mind from the distorted world of Repression up into the clear light of the scientific point of view. By bringing to consciousness what was formerly buried in the Unconscious, it makes possible the interaction and the mutual modification of opposing thoughts and tendencies which were hitherto inaccessible to one another, it enables the patient to look clearly and without prejudice both upon the outer world and into his own soul and at the same time produces co-ordination and integration where formerly there had been dissociation. In so doing, it would appear to throw a new light upon the biological value of consciousness as an integrating mechanism (which would seem to correspond to that of the cortical apparatus on the physiological side) and to indicate also the moral value of *conscious control* over our thoughts, feelings and actions.

The satisfactory solution of Conflict, as revealed by Psycho-analysis, would appear to consist in bringing both the opposing tendencies into the focus of consciousness, recognising them for what they are, both in themselves and in their consequences, without any attempt at covering up or slurring over any of their aspects, either from intellectual laziness or from any painful feeling that they may arouse. The whole experience and the whole of the mental ability of the personality is then available for deciding between the conflicting claims—a state of affairs which is impossible as long as there exists any considerable degree of

Repression. In the light of this experience and with the help of this ability, it is then usually possible to arrive at some morally and intellectually satisfactory solution of the Conflict.

If we attempt to examine more closely the exact nature of the conscious processes whereby particular desires or tendencies are thus brought into relation with the other dominant tendencies of the personality, it would appear that the mechanism consists, at any rate to some extent, in the logical process of subsuming the end or goal of each tendency under some higher end or goal which appears immediately desirable or undesirable, *e.g.* (in the case of desirableness) health, success in life or in some particular enterprise, self-respect, the good opinion of other people or of some one of them, or else some accepted moral precept, such as justice, kindness, chastity. As a result of this process the mind is able to see clearly the manner in which the particular desires and tendencies in question are related to the other interests of the personality, and these other interests are in turn brought to play upon the particular desires, in a way which is otherwise impossible.

It is obvious that in such a case the process cannot logically come to an end at this point and that these higher ends must often be themselves called into question and decisions made between them This involves the subsumption under still higher ends until we come at last to some conception of the highest good of all, the *summum bonum*. Theoretically perfect conduct on this principle would thus consist in a completely harmonious series of means to ends, all duly subordinated to the highest end of which the mind in question could conceive. It would in fact be conduct such as would be generally recognised by teleological systems of Ethics as both intellectually and morally the most desirable.

Of course, in actual practice the highest harmony would be very far from this ideal. To the best of human minds any conception of the ultimate goal of human endeavour is but vague and shadowy. To the majority such a conception is entirely foreign, and even the subordinate principles of moral conduct are but imperfectly understood. In most cases only a few steps in the above outlined process of subsumption are carried out. It may often happen that the mere realisation of the more immediate consequences of the action will suffice to solve the conflict. In nearly every case the process will stop far short of its logical termination.

Nor indeed do all the stages of the process invariably take place within the limits of consciousness. In the more rapid and startling cases of cure by Psycho-analysis, the pathological symptoms often vanish when only a partial understanding of their meaning has been attained and long before the patient has had time to grasp the true nature and ethical significance of the conflict as the result of which they have arisen. In these cases it is evident that a large part of the processes which lead to the solution of the conflict and the abolition of the pathological manifestations must have taken place subconsciously. A cure of this kind presents a certain appearance of inexplicability, which has probably been responsible for the idea that the mechanism at work has been suggestion. This view however is repudiated by all those having any considerable experience of Psycho-analysis, who maintain that even in such cases the cure is brought about by the removal of repressions and the consequent modification of the repressed tendencies by other psychic influences to which they had previously been inaccessible[1]. It is probable however that these cases (satisfactory though they may be from the point of view of the immediate alleviation of the patient's sufferings) represent a comparatively undeveloped form of the process we are here considering—a form which in the long run affords a less satisfactory solution of Conflict than that in which the process is more conscious[2]. For this reason we are perhaps justified in regarding the fully developed form of the process as a more or less completely conscious one. By so doing we can contemplate more easily the essential characteristics of the process and the manner in which it differs from Repression. It becomes all the more necessary however to keep in mind the fact that in the majority of cases the process only tends to approximate to and does not actually attain the stage of conscious explicitness here indicated.

The logical incompleteness of the process of conscious deliberation is not however in reality quite so serious as might at first appear. For even if complete harmony and co-ordination of behaviour were possible, the functioning of the mind is such that the logical process of subsumption would be carried out to its end in consciousness only in a very few cases of exceptional difficulty. In all other cases the mechanism of the Determining Tendency, upon which so much light has been cast by recent experimental studies[2], would ensure a logical consistency of choice and behaviour without the necessity of a frequent appeal to consciousness to supply reasons for the choice. The logical implications of our behaviour, though they *could* be made explicit in a perfectly integrated mind, would nevertheless for the most part remain implicit and would only appear clearly in consciousness when occasional problems of unusual complexity or interest necessitated a review of the relevant facts in the light of ultimate principles.

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[1] Ernest Jones, "The Therapeutic Action of Psychoanalysis," in *Papers on Psychoanalysis*, 1913.

[2] Cf. N. Ach, *Uber die Willenstätigkeit und das Denken*, 1905.

V.

As a result of this subsumption of the ends of the conflicting tendencies under higher or remoter ends or principles, there is brought about, where the process is successful, a re-adjustment of the relative forces of the tendencies, in virtue of which the conflict may be solved.

The actual nature of this re-adjustment will vary considerably from case to case. Often it would seem that one of the opposing tendencies loses all its power relatively to the other, in fact practically ceases to exist any longer as a tendency to action. This, for instance, would probably be the case in the following very simple example. Let us suppose that an article that we require can be bought for five shillings at a shop near our home, but is obtainable at the cheaper price of four shillings at another shop in a more distant part of the city. Here there might very well be a conflict between the desire to buy the article cheaply and the desire to save the time, trouble and expense of travelling to the distant shop and back again. If, now, on due consideration of all the relevant facts, we find that the cost of travel more than covers the difference in price, this fact will practically annihilate the motive to buy the article at the distant shop, we no longer experience the smallest desire to go to that shop and we can proceed to make the purchase in our own neighbourhood without any hesitation. In just such a way will the consideration of the facts and their consequences and of the general principles involved in the alternative courses of action, solve the conflict in more complex cases by making one course appear definitely more desirable than the other and causing the less desirable alternative to disappear from the mind as an impelling motive.

In other cases the psychical process may take the form of realising, in the light of the further facts and principles elicited during the conscious deliberation, that there is no such fundamental opposition between the tendencies involved as there at first appeared to be, and that consequently both tendencies may be allowed to find expression —at any rate to some extent. Thus, in our example, it may appear that the articles sold at the two shops are not really the same and it may be desirable for us to possess both articles. Thus too, in a more complex case, consideration may reveal to us that neither of the apparently conflicting tendencies is really opposed to the dictates of the highest morality: on the contrary, it may be our duty to indulge both of them, as in the case of egoism and altruism, work and recreation.

Very frequently, again, conscious deliberation will reveal just where and in what respect the conflict lies and will show that in many ways the tendencies are not really so much opposed to one another as they at first appeared to be. Here the process is largely one of discrimination, and it is just here that conscious deliberation differs very markedly from Repression. Repression is apt to extend from the element of thought which was originally its object to other elements that are associated with it, the associations along which the repression extends being often, moreover, of the superficial type so characteristic of the Unconscious. So far from revealing just how or why a particular feeling or tendency is out of harmony with some other feeling or tendency of the personality, it confuses the issue by producing an apparent opposition between the whole masses of thoughts and feelings which happen to be associated with the original feelings or tendencies, but between which there exists no logical incompatibility. Conscious deliberation, on the other hand, with its discriminative power, will often reduce the original conflict to much smaller dimensions, by showing that in most respects the two tendencies are perfectly compatible with one another. It is along these lines that the solutions of many important moral conflicts are to be found; more especially perhaps such conflicts as those connected with the problems of sex, where wholesale Repression has made many minds incapable of any rational distinction between right and wrong, since any manifestation whatsoever of the sexual instinct and any serious preoccupation with these manifestations (otherwise than to condemn them) are alike regarded as Taboo. No considerable moral progress can be made in such matters till repressions are sufficiently removed to allow of unprejudiced examination as to why and to what extent certain manifestations of the instincts or tendencies in question are undesirable, and again why certain others are desirable. When unbiased consideration of these questions becomes possible, it is clear that opinion and behaviour in such matters will attain a much higher degree of specificity and differentiation than is possible at the stage of development in which Repression plays a leading *rôle*.

In other cases, again, the process of conscious deliberation, by removing repressions, may pave the way for Sublimation, so that the conflict may be solved by one of the conflicting tendencies assuming a sublimated form, in which shape it no longer meets with opposition. The actual process of association, in virtue of which the Sublimation takes place, is, as we have seen, usually an unconscious one. Nevertheless the process of conscious deliberation may be of use in this respect in at least two ways. In the first place by the removal of repressions—particularly such repressions as affect not only the original ideas at which they were aimed but others which are associated with them—and the consequent setting free of the repressed energy to seek new channels which were hitherto not open to it. In the second place by the proper understanding of the nature of these new channels themselves. Valuable forms of Sublimation will be realised for what they are, and full opportunity will be granted for their exercise; on the other hand the true nature of undesirable and pathological displacements will also be detected: their worthlessness or foolishness will be more easily recognised and they will disappear in the same way that other tendencies recognised as worthless disappear under the influence of Conscious Control.

Finally, in still other cases, it may appear in the light of conscious examination that neither of the conflicting tendencies should be indulged. Thus, in our previously used example, we may realise on further reflection that the article we contemplated buying is not needed at all. In this case there occurs a removal to another "universe of discourse," the whole question becomes irrelevant and the conflict is abolished by the annihilation of both competitors in the same way as one was done away with in the cases we have hitherto considered.

VI.

Whatever the precise mechanism involved in the solution of conflicts in the various ways we have enumerated, the essential process everywhere consists in the full unrestricted play of the intellectual faculties, leading eventually to the fullest possible knowledge. So important is the part here played by knowledge that, as Prof. E. B. Holt has recently pointed out[1], it would appear to afford considerable justification for the Socratic doctrine of the identity of knowledge and virtue. We are able to act rightly just so far as we see straightly, reason accurately and know correctly. Modern psychologists have, however, an advantage over Socrates in so far as they are now beginning to understand better than was possible to him some of the subtle ways in which our knowledge is defective and our conduct consequently lacking in the quality of virtue. At the same time, by their appeal to the unconscious processes of the mind, they are able to free the doctrine from its character of paradox, which it seems necessarily to possess as long as we confine ourselves to purely conscious happenings. This emerges clearly if we turn to consider more closely the conditions which are responsible for the actual imperfections of our knowledge, in so far as they are of importance for the rational guidance of behaviour.

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[1] E. B. Holt, *The Freudian Wish*, 1916. Professor Holt's main conclusions as to the ethical significance of Freud's doctrines are in many respects similar to those indicated in the present paper.

It will be convenient to make a rough classification of these imperfections under two heads as follows:

(1) Ignorance of relevant facts in the outer world.
(2) Ignorance of relevant mental factors in ourselves.

(1) As regards this head, we may distinguish between (*a*) cases where the lack of knowledge is not attributable to any pathological or quasi-pathological imperfections in the mind of the knower, (*b*) cases where the lack of knowledge is so attributable.

Cases coming under the head of (*a*) are comparatively simple and need not long detain us here. Their occurrence is inevitable owing to the very nature of the human mind and its limited power of grasping its environment, though they may be rendered less frequent and less complete as gradually increasing stores of information become available for the individual and for the race. To take a simple instance, let us suppose (to return to our former example) that we may have no means of knowing the precise difference in the price charged for the same article in the two shops or the precise amount of the fare to the further shop and back. There are certainly many conflicts, both trivial and serious, which are of this nature, and as regards which we have eventually to make a decision on very inadequate information. The healthy reaction to situations of this kind—the one that is most in accordance with the principle of Conscious Control—would of course consist in throwing the whole psychic energy upon that side which—after such deliberation and consideration of the facts as is possible in the circumstances—seems on the whole to be the most desirable. In those cases (distressingly frequent in some minds) where the most careful consideration fails to reveal any appreciable preponderance of desirability on the part of one or other alternative, it is probable there is some further lack of knowledge of the kind to be considered under the second heading (knowledge of ourselves). In this event, the conflict may be solved more easily as soon as this further lack of knowledge is made good. In the comparatively few cases where the advantages of the alternative courses of action would even then continue to appear to be equally balanced, it would seem justifiable to leave the decision to the arbitrament of chance by the frequently adopted device of tossing a coin or by some similar method, on the principle that, whatever course is ultimately adopted, it is better to pursue this course with the whole of the psychic energy available than to waste energy and achieve nothing through a prolongation of conflict and indecision.

Cases of (*b*)—in so far as they are not due to obviously defective intellectual power—are in general more subtle in nature and much more likely to be overlooked. The fuller realisation of their importance which we have gained in recent years is largely due to the studies of the psycho-analytic school. They are in fact merely instances of the working of Repression and Displacement, and in their simpler manifestations are often easily recognisable as such, along with the other cases of everyday psycho-pathology. Thus, in our example, we may forget that the fare to the more distant shop more than covers the difference in the price of the article to be purchased, or we may neglect to take the fare into consideration at all, or we may erroneously suppose that the fare is cheaper than it is, even though its exact amount may be ordinarily known to us. In each case the correct knowledge may be repressed or distorted by an unconscious tendency, in virtue of which we are impelled to give preference to the more distant shop. As far as our unconscious tendency is concerned, we *should like* to go to the more distant shop, therefore the facts are made so to appear that it *is* more desirable to go to that shop.

In so far as mechanisms of this sort are operative, our environment is distorted in order that it may be made to appear in conformity with our wishes rather than as it really is. It is from this point of view that Freud[1] has spoken of two principles of mental process: one, the Pleasure-Principle, which seeks to make reality appear as we would wish it; the other, the Reality-Principle, which seeks first to apprehend reality as it is[2].

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[1] "Formulierungen über die zwei Principien des Psychischen Geschehens," *Jahrbuch f. Psychoanalytische und Psychopathologische Forschungen*, III. 1.

[2] The terms would appear to be unfortunately chosen, for at bottom both principles are really pleasure principles, inasmuch as they seek the gratification of some wish. The

It is only in so far as the Reality-Principle is at work that we can enjoy the fullest and most accurate knowledge of our outer world of which our intellect is capable: and this principle will only come into full operation where there are no unconscious tendencies working beyond reach of our control. It thus appears that to know the outer world, we must first know ourselves, and that only in so far as we are aware of our own tendencies and feelings can we be immune from the distortions of reality which these tendencies inflict upon us. The increasing integration of our own mental life brought about by Conscious Control provides us also with the means of more exact adaptation to our environment and of more extended and accurate modification of the environment to suit our own purposes. Thus both pure and applied science may profit from the increased understanding of our own selves, and it is not at all impossible that a thorough knowledge of our own inner life (such as can only be acquired by a process of psycho-analysis) may come to be regarded as an indispensable, or at any rate a highly desirable possession for all those who aspire to the scientific study and the practical control of the forces of the outer world.

(2) In view of this result, all the greater importance attaches to the study of the second class of imperfections in our knowledge—*i.e.* those imperfections which are due to ignorance of ourselves. It is here that Psycho-analysis or some other method of studying the Unconscious becomes an almost necessary instrument for the acquirement of the higher degrees of integration and of Conscious Control. If we carefully examine our behaviour in the light of the conscious motives that we can introspectively discover, we shall frequently find that there appears to be no motive present even in the case of quite important decisions, or else that such motives as we can find do not seem logically adequate. Thus, in our oft quoted example, we may find ourselves firmly decided to go to the more distant shop, although we are not aware of any adequate reason for this decision. In this case the reason is to be found in some unconscious (and therefore probably repressed) desire or tendency which finds greater satisfaction in going to the distant shop than to the nearer one. By means of Psycho-analysis we may

one however seeks this gratification by the facile process of making reality appear to be such as this would demand, the other by the much more laborious method of first apprehending reality as it is and then endeavouring actually to modify reality so as to bring it into conformity with the wish. From this point of view Bleuler's term "autistic" as applied to the first of these principles would seem in some respects to be preferable ("Das Autistische Denken," *Jahrbuch f. Psychoanalytische und Psychopathologische Forschungen*, IV. 1).

overcome the Repression and make the reason conscious. It may then be found perhaps that by making a journey to the more distant shop, we shall avoid some unpleasant duty at home, such as writing a disagreeable letter or meeting some unwelcome visitor. Or again there may be some attraction at the distant shop not strictly connected with our business there. It may be, for instance, that in this shop we shall have an opportunity of meeting some acquaintance (probably of the opposite sex) whom, for various reasons, we had determined not to see again, but who continues to exercise an attractive power in the unconscious levels of our mind. It is evident that in such cases, so long as the motives remain in the Unconscious, we are completely at their mercy. We can form no true estimate of the consequences of postponing the writing of the letter, neglecting to receive our visitor or becoming more intimate with our friend of the shop, for these—the real motives of our decision—are inaccessible to our consideration. It is only when the motives become conscious that we can gauge accurately the moral responsibility attaching to the acts to which these motives impel and bring these motives themselves into due relation with the other aspects of our personality.

Here it becomes necessary, in concluding this portion of our subject, to emphasise an important point as regards the relation of Conscious Control to knowledge of ourselves—a point which has been brought out very distinctly in the history of Freud's psycho-analytic method. It is not sufficient for the purpose of Conscious Control that we should bring to consciousness merely the cognitive aspects of our unconscious tendencies; the *feeling* that normally attaches to these tendencies must also be experienced. This is shown clearly in those cases, not infrequent in psycho-analytic practice, where discovery on the part of the patient of the meaning of his pathological symptoms fails in itself to bring about a cure: in such cases it would seem that the cognitive aspects of the repressed tendencies have become, as it were, de-emotionalised and that the feeling that should normally accompany them has become dissociated and either flows on to some other object by a process of Displacement or else remains altogether in a state of Repression. Here it is plain that the really essential aspects of the repressed tendencies, *i.e.* their affective and conative constituents, are still below the threshold of consciousness and (as might have been anticipated in these circumstances) experience has shown that a cure can only be effected when analysis has gone far enough to make the subject *feel and not merely know* the tendencies in question.

In so far as this dissociation of affective and cognitive elements takes place during the actual process of bringing unconscious tendencies to consciousness, it is untrue to say that mere recognition of the existence and nature of our unconscious tendencies is all that is required for Conscious Control. In this respect the results of Psycho-analysis are only in accordance with the doctrine of Socrates if we extend the meaning of Self Knowledge so as to include Self Feeling. If, however, we understand Socrates's famous formula "Know thyself" in what would appear to be its only reasonable sense, as an injunction to realise and have under our control not only our thoughts but also our motives and their accompanying feelings, it would seem that we are justified in making this extension; for the only way in which feelings can be realised is by being felt.

If this is so, we may, in the light of modern Psychology, interpret the Socratic precept as meaning that we should know our own Unconscious, or, to be more precise, bring to Consciousness the content of our Unconscious. In so doing we at once remove one of the principal obstacles to virtue, and at the same time clear away the great theoretical difficulty of the Socratic position—the occurrence of situations in which "video meliora proboque, deteriora sequor." The explanation of these situations is, for the most part, to be found either: (1) in the fact that the full motives impelling to what is to Consciousness the inferior course of action are present in the Unconscious only, or else (2) in the fact that the feeling that should normally accompany the thought of the 'better' course of action is not present; there is a dissociation of feeling and cognition of such a kind that what appears cognitively to be the better course is inferior so far as feeling is concerned; this course of action is only known and not felt to be the better. In either case there are important factors in the situation which are beyond the influence of conscious deliberation and control. In so far as we can succeed in bringing to the focus of Consciousness *all* the thoughts, feelings, emotions and tendencies aroused in us by a particular situation, just so far shall we be able to bring about a satisfactory solution of the conflict between opposing tendencies, in one of the ways indicated above—a solution which is at once the wisest and the best of which our personality is capable.

VII.

In the course of our examination of Repression, Displacement and Conscious Control it has become apparent that the three mechanisms represent to some extent successively more evolved methods of dealing with a conflict between opposing psychic tendencies. In view of this result it is interesting to study the working of these mechanisms in the light of some recognised standard of development which is applicable to conduct and to mental life. For this purpose we may perhaps accept Herbert Spencer's famous definition of Evolution, as "a change from a relatively indefinite incoherent homogeneity to a relatively definite coherent heterogeneity through successive integrations and differentiations." The two principal directions of Evolution here indicated—integration and differentiation—can both be traced in the transition from Repression through Displacement to Conscious Control.

It is evident from our consideration of Repression that this state represents a comparatively low degree of integration, in so far as Repression necessarily implies a certain amount of dissociation. In the more complete cases of Repression this dissociation may manifest itself only in a diminution of the amount of energy available for the ordinary purposes of mental life (since the energy inherent in the repressed tendency is not at disposal for such purposes), though usually it will be accompanied also by a number of more specific inhibitions which, though not very apparent to a merely superficial examination, may nevertheless have serious effects on mental and moral development. In the less complete cases it may give rise to more marked inhibitions both of thought and conduct; the subject of such Repressions is at war with himself, he is apt to be impeded in all he thinks and does and, in bad cases, almost everything that he accomplishes has to be carried out in the face of severe opposition. In Repression in its pure form the contending forces are arrayed against each other as irreconcilable enemies, between whom there can be no *rapprochement*, mutual modification or understanding. A mind which suffers from many such repressions obviously possesses but a low degree of integration.

Nor is it much better off as regards differentiation. For, as we have seen, Repression tends always to spread from the element at which it was originally aimed to other elements which are associatively connected with the original element. Thus there is produced a state of affairs in which both moral and intellectual discrimination are rendered impossible as regards all that pertains to the repressed complex. In this stage a person is totally unable to distinguish just how or why a thing is wrong or undesirable. He illogically and unjustifiably extends his Taboo to everything which appears to have some connection (however fanciful or far-fetched) with the original object of his moral abhorrence, and in so doing turns his back on much that he can ill afford to lose.

Turning now to Displacement, it is obvious that this mechanism represents a higher stage of integration than that of pure Repression. There is no longer a complete dissociation between the conflicting tendencies within the mind. The conflict is solved by means of a compromise, the two forces interacting so as to express themselves in a way that, as far as possible, gives satisfaction to both. There is no longer the waste of energy that is inevitable in pure Repression, and the mental life is able to develop harmoniously and without any severe inhibitions.

The essential weakness of Displacement, however, lies in the fact that, in all its most important aspects, it is a process that goes on almost entirely in the Unconscious and partakes therefore of the blind, groping, non-rational and undiscriminating character which distinguishes unconscious processes in general. It has not the advantage of those powers of relatively unbiased judgment, delicate discrimination and accurate adjustment of means to ends, which are at disposal in the process of conscious and deliberate choice. Nevertheless we have ample justification for regarding Displacement as a process of the greatest importance in mental evolution, for there can be no doubt that, in its more beneficial forms, it represents the means by which a great part of our moral progress—especially that in early life—has necessarily to be brought about.

It would seem fairly certain that, from our present point of view, the highest stage of ethical development is represented by the principle which we have termed Conscious Control. We have seen that in its perfect form this principle presents the possibility of more or less complete integration of conduct and of mental life. In so far as the principle is capable of thorough application, the psychic life of the individual will proceed with the highest degree of harmony that is possible in view of the conflicting nature of many of the ultimate conative trends of the human mind; each instinct and desire will receive the maximum of gratification that is compatible with the similar gratification of other instincts and desires; each tendency will fall into its proper place in the general scheme of life, and the whole of the mental energy will be available for any enterprise that may be undertaken. Similarly, the conduct that is based on Conscious Control will be capable of the highest degree of differentiation, since conscious discrimination is capable of picking out the desirable and undesirable elements in any total situation, and of guiding conduct accordingly; whereas Repression, as we have seen, would lead to a totally undifferentiated behaviour as regards all the elements of the situation.

It is interesting to observe also, that Conscious Control still appears to be the most evolved mechanism for the guidance of conduct, when judged by a somewhat different standard, *i.e.* that of the "adaptation of inner to outer relations" in which, according to Spencer, vital activity itself consists. We have seen how the presence of repressed tendencies is liable to distort our appreciation of the outer world, and so to prevent our adjusting ourselves accurately to it. This mal-adaptation to the environment may extend from the small lapses, errors, and oversights which have been included by Freud under the heading of the "Psychopathology of Everyday Life," to the almost total loss of touch with the real world which may be exhibited in certain forms of insanity. But in whatever degree it may be present, *some* distortion of reality is almost necessarily involved in Repression. A true view of the outer world and the consequent possibility of accurately adjusting ourselves to, and dealing with, this world can only be gained by the overcoming of Repression and the acquirement of the full psychic integration which follows on Conscious Control. The transition from Freud's Pleasure-Principle to his Reality-Principle would thus accurately correspond to a greater adaptation of inner to outer relations in the Spencerian sense.

In fact, it is remarkable with what ease the principal features of Freud's psychology, when considered from the point of view of development, appear to fit into the Spencerian scheme—a further proof, if this were needed, of the validity of the main principles of both investigators. While Spencer formulated the main outlines of mental development in a way that seems likely to hold good, in most of its essential features, for the whole future advance of Psychology, Freud has incalculably added to our knowledge of the details of the process, and has, more particularly, enriched our conceptions as to the parts played in this process by conscious and unconscious factors respectively.

VIII.

Having surveyed the Freudian mechanisms with which we are here concerned in the light of the criteria set up by the greatest of evolutionary psychologists and moralists, we will, in conclusion, very briefly indicate what seems to be the place of these mechanisms in the actual development of human life, thought and institutions. Here again it will appear that the tendency of Evolution is towards a more thorough conscious control of thought and action, and an abandonment of the more primitive attitude involved in Repression.

The close dependence which we have noted between Conscious Control on the one hand and knowledge and unrestricted intellection on the other makes it evident that the higher degrees of Conscious Control are only possible in higher types of mind. In comparatively undeveloped minds we should expect to see conduct determined largely by Repression and Displacement. This expectation is clearly fulfilled if we compare children with adults, or savages with civilised and educated men. The mind of the young child, with its unstable and incoordinated desires and tendencies, its inability to realise the true meaning of its impulses or to trace out their remoter consequences, is obviously lacking in many of the most essential features of Conscious Control. Recent researches into Child Psychology indicate that Repression, on the other hand, may be found quite early in the child's mind and that it is the means by which the earliest moral progress is achieved[1]. The growth of Repression can be traced during the years of childhood, together with that of Displacement[2]; but both mechanisms, in the course of mental development, become after a time limited and controlled by the higher conscious processes, the unrestrained increase of Repression giving rise, not to further moral advance, but to the retrograde condition of neurosis. It would thus appear that Repression is an essential instrument of progress in the early stages of development, but is destined afterwards to be relegated to a relatively subordinate position among the controlling mechanisms of the mind.

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[1] Cf. Freud, "Analyse der Phobie eines fünfjahrigen Knaben," *Jahrbuch f. Psychoanalytische und Psychopathologische Forschungen*, I. 1; C. G Jung, "The Association Method," in *Papers on Analytical Psychology*, 1916, p. 132, Ernest Jones, "The Unconscious Mental Life of the Child," *Child Study*, April, 1916.

[2] The gradually increasing influence of Repression and Displacement is well brought out by a comparison of the dreams of young children with those of adults. (Cf. Freud, *Traumdeutung*, p. 92, Ch. 3.) It would seem also to be apparent in the characteristic differences between the dreams of cultured and those of relatively uncultured peoples. (Cf. J. E. Lind, "The Color Complex of the Negro," *Psychoanalytic Rev.* I. 404.)

The savage also, like the child, with his conflicting beliefs and actions, his relative lack of foresight of remoter ends and consequences, and his morality based largely on Taboos for which he can give no adequate reason[1], is obviously to a considerable extent dependent on Repression, and exercises but little Conscious Control in comparison with individuals belonging to more civilised communities.

Nor indeed has the civilised adult himself progressed so far on the road to Conscious Control as we could desire. We have already indicated the very imperfect degree to which he has attained this Conscious Control in his mental life as an individual. This same imperfection of attainment is manifested no less clearly in his social institutions and beliefs. At the same time the relative predominance of Repression or Conscious Control affords an interesting indication of the relative cultural status of such institutions and beliefs.

Thus it is clear that the more autocratic forms of government and social restraint tend in the main to produce a state of mind more favourable to Repression than to Conscious Control. All that is required of a man under autocracy is blind obedience to command, without question of the Why or Wherefore, and in so far as his conduct is determined from above in this way, his attitude towards the moral sanctions imposed by authority is apt to be very like that of a young child towards the prohibitions of its parents[2].

Under democracy, on the other hand, there is usually a stronger motive for a man to exercise his powers of criticism and deliberation, and thus to lay the foundations of Conscious Control. Indeed, it would seem that the success of a democracy must be largely dependent on the degree in which its members are able to bring Conscious Control to bear upon political problems, and that the characteristic difficulties with which democracies have had to struggle have been to a great extent due to deficient exercise of Conscious Control in dealing with these problems. It is obvious that the field of politics—in which the Suggestions and Taboos imposed by our human environment are unusually strong and insistent, and are, it would seem probable, aided by the operation of a special instinctive mechanism which makes us peculiarly receptive to them[3]—is one in which the application of Conscious Control appears to be a matter of exceptional difficulty, and in which our judgments and inferences are most frequently distorted by repressions[1].

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[1] Cf. Freud, "Totem und Tabu," *Imago*, I. 23.

[2] Modern psycho-analytic research seems to show that this is no mere simile.

[3] Cf. W. Trotter, *Herd Instinct*, 1916, as the latest addition to the growing list of important works on this subject.

Unfortunately, moreover, we have as yet scarcely begun to realise the desirability of clear vision and unbiased judgment in this field; for we are still apt to cherish institutions and beliefs which obviously depend largely on Repression. Thus the system of party politics requires that we should repress all thoughts that may arise as to the defects of our own party or the merits of the opposing one, and is definitely hostile to any unprejudiced inquiry as to where the real good of our country may be found. The conduct of a state which is guided by two opposing parties is, indeed, very similar to that of an individual who is the prey of two opposing tendencies of approximately equal strength, and state management on these lines suffers from disadvantages very similar to those which we have already studied in the case of the individual. Similarly, in the case of international politics, the cult of the narrower forms of patriotism inevitably leads to a distortion of all facts concerned with the relations between nations, and among the causes of War itself, not the least is to be found in the mental blindness induced by Repression acting on whole peoples and races.

In the equally important sphere of social and economic questions we find the same mechanism at work. The emotional tendencies aroused, for instance, by problems connected with class, wealth or sex, springing as they often do from sources buried in the Unconscious, effectually prevent, by the repressions to which they give rise, any clear and unbiased intellectual appreciation of these problems, and only in proportion as we lay bare these buried sources of emotion will such appreciation become possible. Even in the scientific treatment of these subjects the influence of Repression is still clearly to be traced, and the reason why sciences such as Sociology and Economics, which are more directly concerned than any others with the welfare of human society as a whole, have made less progress than the so-called Natural Sciences, is probably to be found very largely in the fact that the former sciences have been pursued more often in the spirit of prejudice and partisanship than from disinterested love of truth, through which alone relatively clear appreciation of facts and unbiased judgment are rendered possible[2].

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[1] Cf. W. Lippmann, *A Preface to Politics*, 1914.

[2] Thus it would appear to the present writer that whole branches of Economic Science are retarded by failure to take due account of one of the fundamental principles of that science—the tendency of population to press upon the means of subsistence. This

In Religion and Philosophy, no less than in Science and in Politics, it is possible to trace the influence of the mechanisms with which we have been here concerned. Psychological study has of late years begun to reveal something of the nature of the multitudinous unconscious factors which largely determine the character of religious experience and doctrine. Where unconscious tendencies are in such force, it is clear that there can be no very high degree of conscious integration; and it is indeed obvious enough that religious thought and conduct, though in other respects it may attain a high degree of social value, nevertheless departs very widely from the principle of Conscious Control. Religion distorts our view in two ways: positively, by remoulding the Universe "nearer to the heart's desire" (Pleasure-Principle), and negatively, by shutting our minds to everything that is in flagrant contradiction with this remoulded Universe. Dogmatic religion must be for ever incompatible with the complete integration of mind and the complete triumph of the Reality-Principle which would seem to constitute the highest stage of mental development according to the principles we have been here considering.

Philosophy itself however, although its professed aim may be the attainment of truth by the exercise of pure intellect, neglecting the appeal of the emotions, is yet far from being immune from the influence of repressed unconscious factors. Here again it is only in so far as we know ourselves that we can know the Universe[1]. As regards Moral Philosophy, with which we are here perhaps more particularly concerned,

principle, first clearly enunciated by Malthus, has, in the hands of Darwin, Wallace and later investigators, proved of far-reaching consequence in biological science, but is for the most part simply neglected or else summarily dismissed by contemporary economists, though the principle is obviously of the greatest importance for many of the most pressing social and economic questions of the day, such as those connected with the declining birthrate, the increased cost of living, etc. (Cf. C. V. Drysdale, *Wages and the Cost of Living*, London, 1913; J. C. Flügel, "Ethics and the Struggle for Existence," *Int. J. of Ethics*, 1915.) The astonishing treatment that recent Political Economy has given to a doctrine that has won general acceptance in a sister science affords an interesting example of the baneful influence of Repression upon scientific thought; for in this case it is fairly easy to see the general nature of the principal repressions that are at work. They would appear to be: (1) of a sexual character, the whole subject being intimately bound up with the most delicate problems of sexual morality and hygiene, problems which very few are able to face in a scientific spirit, (2) of a political character, connected especially with class feeling and prejudice, and with the desire—on the one hand to make sure of a plentiful supply of cheap labour, on the other hand to lay the blame for poverty upon such institutions as Capitalism or Private Ownership.

[1] Cf. von Winterstein, "Psychoanalytische Anmerkungen zur Geschichte der Philosophie," *Imago*, II. 175. E. Hitschmann, "Schopenhauer," *Imago*, II. 101.

it would seem that the teleological systems of Ethics represent an advance from our present point of view over the Moral Sense and the Intuitionist schools, in so far as they are more in accordance with that logical hierarchy of means to ends which we saw to be inherent in the principle of Conscious Control. Those moralists who see the manifestations of Conscience or the Moral Sense in each particular moral judgment are obviously at the mercy of any tendency which may momentarily acquire the upper hand, and are thus liable to be led to great inconsistencies both of thought and action: while even those to whom the moral intuition manifests itself in some more general form as Duty do not attain the highest degree of Conscious Control, inasmuch as the conception of Duty itself is exempt from conscious criticism and is liable therefore to become the hiding place of unconscious tendencies. Even within the teleological systems themselves, however, the influence of Repression may still be found, and the fact that there is yet so little general agreement as to the nature of the Ultimate Good indicates that the thought on this subject has probably to some extent been biased by the unconscious complexes of the individual thinkers. No more here than in other departments of thought can we hope to possess a true and coherent body of doctrine unless we have conscious control of the mechanisms in the human mind through which this doctrine has to be evolved.

It would appear from the above considerations that the progress of culture as manifested in human beliefs and institutions reveals a tendency to pass from a mental state characterised by strong and frequent repressions to one in which Conscious Control plays an increasingly important part; and that, in virtue of this tendency, the extent to which any belief or institution is correlated with Conscious Control may afford a useful and interesting indication of the cultural status of that belief or institution. Our brief sociological review thus confirms the result of the earlier part of our investigation as regards the general importance of Conscious Control for ethical development. It would appear to be a general rule that moral advance, both in the individual and in the race, must be accompanied by an increasing mental integration and differentiation, which it seems to be the peculiar power of consciousness to bring about. Conscious Control therefore, in the course of such advance, tends constantly to supplant the more purely unconscious mechanisms of Repression and Displacement.

IX.

Without in any way wishing to detract from the importance of this, our main, conclusion, we shall do well, nevertheless, to keep in mind the limitations of the principle of Conscious Control.

In the first place, it is a purely *formal* principle. It is concerned only with the interaction of mental tendencies and not with the ultimate nature of these tendencies themselves. Conscious Control may indeed, as we have seen, so modify mental tendencies that they may be said to cease to exist in their original form. Further than this, however, it cannot go. It does not supply, nor can it either augment or diminish, the psychic energy inherent in these tendencies themselves—the *libido* or *élan vital* which is the ultimate source of mental life. In all that we have said we have been concerned merely with the machinery and not with the motive force of mind.

Secondly, it is well to keep in view what has already been said as to the manner in which the function of consciousness in voluntary action is limited by the mechanism of the Determining Tendencies, in virtue of which a conscious decision, once taken, will tend to work itself out without the need of further conscious effort or intervention. From the standpoint of Conscious Control, however, this is not a source of weakness but rather of strength, since it is at once a means of bringing the Unconscious into subjection to the Conscious and of sparing Consciousness the task of constantly supervising the execution of its own decisions, thus setting it free for its more essential function of the formation of decisions based on reasoned and unprejudiced deliberation.

This brings us to a third point connected with the limitation of our principle. The existence of the Determining Tendency as a normal method of carrying out voluntary decisions serves to remind us, if this were necessary, that the principle of Conscious Control does not signify the substitution of the Conscious for the Unconscious in all departments and functions of mental life. On the contrary, even if Conscious Control were theoretically perfect, the greater part of conduct would still fall largely within the province of the Unconscious. The mechanisms of Repression and Displacement would continue to be at work[1], but they would be under the control of, or at least in harmony with, determining tendencies originating from Consciousness and would therefore be acting in accordance with, and not independently of or contrary to, the results of conscious decision. At the highest stage of mental and moral development Consciousness appears, not as a substitute for the operation of the Unconscious, but as a higher controlling mechanism, through which the activities of the Unconscious are differentiated and co-ordinated for the benefit of the organism as a whole.

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[1] As regards the probably very important function of Repression in the normal mental processes of everyday life, cf. Ernest Jones, "The Theory of Repression in its Relation to Memory," this *Journal*, VIII. 33.

Without losing sight of these limitations of the principle of Conscious Control, it would nevertheless appear to be true that in this principle we possess a guide of great value for the study and direction of moral and social phenomena. In an age like the present, when the human race is making vast strides in the control of the physical forces of the outer world but is relatively far behind in the understanding of its own nature and of the factors inherent in this nature upon which its happiness and well being must depend, any such guide should be welcome to all who have the future progress of humanity at heart. It may be that the light that Freud has thrown upon the psychic principles involved in moral development will prove not the least of the services that modern science has performed for suffering humanity.

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THE EXPERIMENTAL STUDY OF BINOCULAR COLOUR MIXTURE. I.[1]

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1. *Historical.*
2. *Binocular combination of like colours.*
3. *Binocular combination of different colours.*
4. *Effect of background.*
5. *Effect of size of coloured surfaces.*
6. *Influence of contours.*

THE stimulation of corresponding parts of the retinae by light of different wave-lengths or of different intensities produces sometimes an alternation of the colours seen by each eye, sometimes a more or less steady mixture. The conditions which affect such binocular rivalry and mixture are so complex that contradictory results have been reached under apparently similar conditions; and for this reason, perhaps, most writers on the topic have been concerned with descriptions of procedure and results more than with their explanation. It is the aim of this paper to give a short historical summary of methods and to describe some experiments on binocular colour mixture.

1. HISTORICAL.

One of the first experiments was performed by Haldat (21):

> Holding before each eye a quadrilateral prism filled with some coloured liquid he looked at a well-lighted piece of cardboard 200 cms. in diameter from a distance of two or three metres, and saw the colour that would have been produced by the mechanical mixture of the coloured substances in the prisms.

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[1] Part of a thesis approved for the Degree of Doctor of Science in the University of London.

To find whether similar results were given by pigment colours he placed squares of paper painted in distemper on a board with a thin piece of wood fixed at right angles to it along its centre. By holding the board close to his face so that the vertical strip was equidistant from both eyes he was able to superpose one colour on another. He was not so successful in combining these pigment colours as he had been with transmitted light. Some colours, *e.g.* blue and yellow, did not combine, or did so only imperfectly. This led him to think that there were affinities between colours analogous to those between tones, that "just as there were sounds whose association was disagreeable because their relations were difficult to seize, so there were colours whose heterogeneity made association laborious and consequently disagreeable" [(21), p. 401].

These experiments have been repeated by many observers, with differing results. Dove (14) produced neutral grey or white by combining complementary polarisation colours in the stereoscope. The experiment succeeded, however, only when the colours were of similar intensities. With pigment colours or absorption colours it failed entirely unless they were very nearly alike. He found great variations in the results given by different observers, and attributed it to the peculiar nature of the combination. He thought that it was possible to attend to either or both of the uniocular images, and that a mixed colour was always seen at the transition from one to the other and in those parts of the field where the two colours were adjacent, for one colour passed gradually into the other. These results were confirmed by Seebeck (46), Foucault and Regnault (20), Brucke (6), Hering (24, 25), and others.

On the other hand Meyer (33, 34), Volkmann (51), and Helmholtz (23) were unable to confirm the results of Haldat and Dove. They failed to combine two colours binocularly so as to get a resultant like the mixture obtained by mixing them on the same part of one retina. Helmholtz maintained that what he saw depended on the direction of his attention, that he could voluntarily see which colour he wished to see, but that when both were seen simultaneously, the colour of the binocular field was not like that of the corresponding uniocular mixture.

The difference between binocular and uniocular mixtures he demonstrated by means of two squares of red paper and two of blue arranged so as again to form a square and placed so that the edges of squares of the same colour were not contiguous. In front of these he held a Nicol's prism so that part of the upper squares overlapped part of the lower and gave a rose-coloured band, a physical mixture of red and blue. When blue, rose, and red were combined binocularly with red, rose, and blue respectively, the difference between the rose band and the binocular combination of red and blue was at once apparent; the latter showed no trace of rose, it was blue and red seen together.

Helmholtz maintained that the so-called cases of binocular colour mixture were illusory, and were due to after-images, induction by the background, and inexact observation owing to the absence of comparison with the uniocular mixture.

Hering, on the other hand, claimed that with due precaution anyone could mix colours binocularly, and suggested that the disagreement among observers was due partly to differences of individual disposition, but mainly to the different conditions under which experiments had been made and to imperfect understanding of what was meant by colour mixture. The colours should be of equal brightness and not complementary: complementary colours could, however, be mixed if they were unsaturated, dull, and of equal brightness (under certain conditions even saturated complementary colours could be mixed). The coloured areas should be small and of exactly the same shape: all contours should be obliterated (if coloured papers were used, the grain of the paper should not be seen), and it was preferable that even corresponding contours should be avoided by throwing the colours out of focus. The mixture, he said, was always intermediate in tone between the combined colours; occasionally, but very seldom, it was exactly like the colour obtained by mixing them on the colour-wheel in the proportions in which they were presented to the two retinae.

Rood (40–43) devised the following neat method of directly comparing binocular and uniocular mixtures. A disc, half of which was green, half red, was placed behind a slide of white cardboard in a stereoscope; in this slide two square holes were cut at such a distance apart that, when observed through the lenses of the stereoscope, they coincided. Behind one opening was the green semicircle, behind the other was the red. After observing the binocular combination, he set the disc rotating and saw the uniocular mixture of red and green through each opening. He used many combinations of colours, and found that the binocular mixture was seldom exactly like that which followed it; generally the former was nearer grey (less pure) than the latter.

Rivers (39) observed binocular colour mixture in after-images by looking steadily at coloured lights and then closing the eyes or looking at a grey surface and making appropriate convergence of the visual axes. In many cases the after-images were not seen simultaneously, but sometimes they gave a mixture such as would have been obtained by mixing the colours on a colour-wheel.

Fechner and Chauveau (8–10) had previously experimented in this direction. Fechner, holding a green glass before one eye and a red glass before the other, had fixated a patch of white paper on a black background. Removing the glasses he found that the paper still appeared white; this was held to prove that the after-images neutralised each other, for, when the patch was doubled by squinting, one image was reddish, the other greenish. This experiment is not, however, conclusive, for, as Rivers has shown, the same effect is given by non-complementary colours, *e.g.* by red and blue.

Rivers suggests that the difficulty which some people have in mixing colours binocularly may be indirectly due to the refractive condition of the eyes. He finds that when the eyes are myopic or hypermetropic, it is easier to get mixture, and attributes this to the blurring of outlines caused by abnormal refraction.

Trendelenburg (49), using spectral colours, could get binocular mixture only when the coloured field subtended an angle of not more than 30 minutes at the eye, and stimulated only the fovea; with larger fields or with peripheral vision rivalry appeared. He found not only that a binocular mixture differed in saturation from a uniocular mixture, but also that the amounts of light of given wave-lengths required to give the same hue were not the same in the two cases. The yellow produced by mixing red and green rays (wave-lengths 671 $\mu\mu$ and 535 $\mu\mu$) seemed slightly less saturated than the corresponding uniocular mixture: this was also true of the purple given by mixing red and violet. The amount of green light (wave-length 535 $\mu\mu$) that had to be mixed with unit amount of red light (wave-length 671 $\mu\mu$)

to give a binocular yellow was very much less than the amount required to give the same uniocular yellow (wave-length $589\ \mu\mu$): a similar result was given by red and bluish-green, but the difference between the two mixtures was not so great.

Welcker (52) and Panum (36, 37) described variations of an experiment previously performed by Du Tour and Wheatstone (53) in which different contours were presented to the two eyes. They found that when different contours, *e.g.* a square and a disc, stimulated nearly corresponding parts of the retinae, there was rivalry between them, and each contour brought with it the colour of the background adjacent to it. When a uniformly coloured field was presented to one eye and a contoured field to the other, the contours predominated in the binocular field and brought with them the colour of their background.

Stirling (48) demonstrated the influence of similar contours in facilitating binocular colour mixture by combining in the stereoscope differently coloured postage stamps printed from the same die. Red and green stamps of exactly the same design gave a black or dull grey binocular image.

In my own experiments I sought to standardise the external conditions by using colours whose saturation, brightness, and hue could by a simple device be varied in a definite and measurable way; and, in order to facilitate comparison of binocular and uniocular mixtures, the conditions were arranged so that both were seen simultaneously.

In the first series areas of uniform colours either alike or different in saturation, hue, or brightness were combined: the later series demonstrate the effect of introducing into these areas contours which are different, or exactly or stereoscopically similar. Incidentally, minor variations of these experiments show the effects of varying the size of the coloured areas, of the brightness of their background, of the duration of the observation, of throwing the colours out of focus, and of several other conditions which may influence binocular combination.

2. BINOCULAR COMBINATION OF LIKE COLOURS.

Do the binocular and uniocular sensations due to a given colour stimulus differ in hue, brightness, or saturation? This question has usually been asked only in connection with brightness, and has been answered in different ways. Valerius[1], Fechner [(18), p. 423], Helmholtz [(23), p. 790], Aubert [(1), p. 286], McDougall [(32), p. 372], and others hold that with binocular vision a surface is sometimes, at any rate, brighter than with uniocular. Valerius says the difference is one-fifteenth of the brightness of the uniocular image; Aubert[2] that it ranges from zero to one-tenth, and that there is no perceptible difference with brightnesses greater than that of white paper in diffuse daylight. Hering [(25), p. 597], Sherrington [(47), p. 50] and others have been unable to detect any difference within the limits of brightness with which they have experimented.

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[1] *Ann. d. Physik u. Chemie*, 1873, CL. 323.

[2] *Physiologische Optik*, 1865, 500.

The importance to be attached to these opinions will depend on the way in which the experiments on which they are based have been conducted; but, unfortunately, few observers say much about this. After one eye has been closed for an appreciable time, say, for a quarter of a minute, while the other has been observing a sheet of white paper, objects appear brighter in binocular vision than to the unrested eye, and less bright than to the rested eye. These differences of apparent brightness do not, however, appear if one eye has not been stimulated more than the other.

In making this comparison dull surfaces should be observed so that light may be reflected equally in all directions, and care must be taken to avoid Fechner's "side-window effect" (19), a difference in the apparent brightness of an object as seen by each eye alternately which is due to a difference in the illumination of the eyes: it is advisable to screen the eyes from all light except that which is reflected from the observed surface.

Even with these precautions apparent contradictions may arise through various causes. One of these is the tendency to confuse clearness of outline with brightness. Binocular discrimination of differences of brightness is in practice finer than uniocular: we can see more detail in a picture when both eyes are used than when one is closed (*cf.* (11)). In comparing the brightness of a sheet of paper as seen by one eye with its brightness as seen by both, one is apt to make the mistake of thinking that because its texture can be detected more easily in the latter case, it is therefore brighter.

When these precautions are taken, there is no evidence of any difference between the brightness of an object as seen by one eye and its brightness as seen by two. I have repeated these observations under all kinds of conditions, with large white surfaces, as a white wall, with small patches of white on black or grey backgrounds, with bright and dim illumination, some hundreds of times both myself and with other subjects, and always with the same results, the brightness of the binocular field is not appreciably different from that of the uniocular; indeed, when any difference is recorded, it is just as often said that the uniocular field is brighter as that it is like or darker than the other.

These experiments I have repeated with patches of black paper and black velvet on a light background and with many colours of various degrees of brightness and saturation, and have always found that not only their brightness but also their hue and saturation remain the same whether one eye or both be used, provided that there is no difference in pigmentation of the refractive media of the eyes.

This is one of the most interesting facts of binocular experience. We must suppose either that the sensory images alternate in such a manner that their combined effect in the binocular field is always the same, or that they are simultaneously present and their binocular product does not differ in brightness, hue, or saturation from either image alone, that there is no summation in respect of any of these attributes.

3. BINOCULAR COMBINATION OF DIFFERENT COLOURS.

In order to find the effect of differences of hue, brightness, and saturation on the binocular combination of coloured fields, I used small areas of pigment colour surrounded by a grey background. The apparatus consisted of discs of coloured papers which were seen through holes in a sheet of grey paper in a stereoscope. It will be most easily described with the aid of a diagram (Fig. 1). A disc of paper (A), 254 mm. in diameter, and another (B), 127 mm. in diameter, were placed one over the other and rotated about the same centre. Another disc (C), also 254 mm. in diameter, was placed about 25 mm. in front of these and so that its centre was 64 mm. to the left of the centre of the large disc (A), and just clear of its edge. By interleaving these with another set of discs, of diameters 254, 127 and 254 mm. respectively, and rotating them at a high speed, their colours could be varied at will.

These colours were observed through holes cut in a sheet of grey paper (D) placed in a stereoscope as shown in the figure. Four circular holes were punched in this sheet by means of a well-sharpened wad-punch, their centres being placed at the four corners of a rectangle whose sides were 38 mm. and 63 mm. (the interocular distance): the holes were all cleanly cut, and 19 mm. in diameter. When this slide was placed in the stereoscope and held in the position shown in the figure, the right eye saw through the upper right hole the colour of the disc B, through the lower that of disc C, and the left eye saw through the upper left hole the colour of disc A, through the lower that of disc C. With both eyes open and the discs rotating, only two small discs of colour were seen one above the other; the upper was a combination of the colours of A and B, the lower was the colour of C. The colour of C was always chosen so that it was a mixture of the colours of A and B in the proportions given on those discs: in this way it was possible to compare the binocular mixture with the corresponding uniocular mixture.

By using several of these slides cut out of grey papers of different brightnesses it was possible to find the influence of the brightness of the background on the combination of the colours.

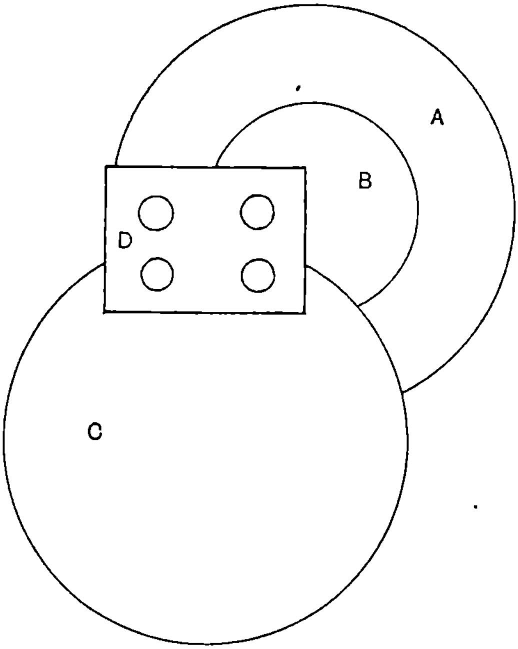

Fig. 1.

The experiments were performed in daylight. The illumination did not vary much, as the observations were all made at the same hour and those in each series were made at one sitting; such variations in the illumination as were present probably did not introduce any serious complications.

The papers from which the discs were cut were those commonly used in psychological laboratories. The colours were fairly pure and the greys neutral.

The choice of colours and of ways of varying them was determined mainly by a consideration of the attributes peculiar to visual sensations, which are generally admitted to be three in number, viz., brightness, saturation, and hue. The brightness of a colour was varied by mixing it with white, black, or a grey lighter or darker than itself. Its saturation was changed at the same time, for it was impossible to keep it constant and at a maximum while the brightness was being varied. The saturation of a colour was varied while its brightness was kept constant by mixing it with an equally bright grey. Any hue could be obtained by mixing colours of different hues in varying proportions.

Only a few of the enormous number of possible combinations were selected for experiment. They may be divided into three groups. In one the colours were alike in brightness and saturation, but differed in hue, the hues being either complementary or non-complementary: in another they were of the same hue and brightness, but of different degrees of saturation: in the third group they differed in brightness. These will suffice to demonstrate the principal facts of the binocular mixture of pigment colours.

The subjects were trained observers with considerable experience in making psychological observations. Three of them, Messrs Jack, Bodin, and the author took part in all the experiments: their results were corroborated by those of about sixty less fully trained subjects each of whom performed a few of the experiments. To all who have assisted in the performance of these somewhat fatiguing experiments I beg to acknowledge my indebtedness: I am also indebted to Dr H. J. Watt for his invaluable advice and assistance.

(a) *Complementary colours of the same brightness and saturation.*

The series of coloured papers usually supplied for psychological experiments seldom contain complementary colours of the same brightness. Of those at my disposal all except red and green differed so much in this respect that when they had been made equally bright by mixing one with white and the other with black, the colours were very unsaturated. Only red and green, a verdigris green, were approximately alike in brightness, and therefore only the experiments with these colours will be reported. Other pairs, however, of less saturated complementary colours gave similar results. The red was a darkish red which seemed to be somewhat purer than the green. Mixed on the colour-wheel, they gave a neutral grey when the disc contained

210° green and 150° red. They were, therefore, presented in these proportions to each eye.

A grey was found (by the flicker method) which was equal in brightness to the green, and this was mixed with the green to reduce its saturation. As the red was perceptibly darker than the green, it was necessary to add to it varying amounts of white as well as the above grey in order to make the colours equally bright. In every case the colours to be mixed binocularly were equal in brightness to the green.

Seven degrees of saturation were chosen, in which disc A (see Fig. 1) contained 360°, 300°, 240°, 180°, 120°, 60°, and 30° respectively of green. Disc B contained not only that amount of red which when mixed on the colour-wheel with the green on A gave a neutral grey, but also white and grey sufficient to make the brightnesses of the two colours equal. Disc C contained red, green, white and grey in the proportions given on the other discs, *e.g.* when there were 360° of green on disc A, disc B was composed of 257° red, 45° white, and 58° grey, while disc C contained 180° green, 128°·5 red, 22°·5 white, and 29° grey. The colour of the lower disc was always the same neutral grey.

The slide in the stereoscope was made of the grey paper which was used for reducing the saturation of the green, and therefore was of the same brightness as all the colours. A black slide and a white one were also used, both in focus and out of focus, in order to find the effect of the brightness of the background and of putting it in or out of focus. Here, however, we shall consider only the results obtained with the grey slide in focus.

In every observation the slide was held for thirty seconds in the position shown in Fig. 1, then it was moved to the right and for thirty seconds was held so that the left eye saw the colour that had been seen by the right and the right eye saw the colour that had been seen by the left, then it was moved back to the first position and held there for another thirty seconds. I shall refer to these positions as (*a*), (*b*), and (*c*). The colours were interchanged in this way to find the effect of duration of the observation and to test Helmholtz's contention that prolonged observation effects such a reduction in the saturation of colours that two different colours may come to look alike.

The subject was instructed not to fixate any point, but to let his gaze wander over the discs and adjacent parts of the slide and to compare and describe the colours of the discs: his observations were recorded, and at the completion of the experiment he described more fully what he had seen, and was sometimes questioned about it.

Each observation was repeated several times, and the usual precautions were taken in arranging them so as to distribute the effects of fatigue, practice, etc. over the whole series.

Table I is a summary of the records of three subjects. In making this table, which represents a general impression of a number of observations (not less than six for each value of the stimulus), slight differences between the observations in positions (*a*), (*b*), and (*c*), afterwards to be described, have been ignored. It indicates both the effect of saturation and individual differences.

TABLE I. *Complementary colours of the same brightness and saturation.*

Left Green	Right Red	Subject B.	Subject D.	Subject J.
360°	257°·1	Little rivalry, but not alike	Vigorous rivalry	Keen rivalry
300°	214°·3	Less rivalry	Rivalry	Rivalry
240°	171°·4	Mixture and a little rivalry	Less rivalry	Rivalry and mixture
180°	128°·6	Mixture and slight rivalry	Still less rivalry	Less rivalry and more mixture
120°	85°·7	Mixture	Mixture and slight rivalry	Rivalry and mixture
60°	42°·9	Mixture Alike	Mixture and trace of rivalry	Mixture with slight change in colour
30°	21°·4	Neutral mixture Alike	Neutral mixture Alike	Neutral mixture

The background was of the same brightness as the colours, and in focus. 'Mixture' here means that for an appreciable interval a steady colour was seen which was intermediate in tone between the uniocular colours, but not necessarily like the uniocular mixture. 'Not like' and 'alike' signify the result of a comparison between the binocular and the uniocular mixtures.

The more saturated the colours, the greater is the rivalry between them. The purest colours displace each other completely in rapid succession; less saturated colours displace each other more slowly and less completely, *i.e.* some parts of the upper disc are red while others are green; still less saturated colours give an almost neutral tint which is never quite still, being sometimes reddish, sometimes greenish, either entirely or in parts; colours of the lowest saturation, greys with a tinge of red or green, give a steady mixture which is like neither of the uniocular colours but resembles that of the lower disc. Before this last stage is reached there is a period of doubt and uncertainty in which the subject suspects that changes are taking place in the upper disc but is unable to decide whether he is changing his attitude to the same colour, calling reddish or greenish what previously he called grey, or whether there is an actual change in the colour such as he gets with greater saturation. Often at this stage he thinks he detects red or green in the upper disc for an instant while he is looking at the lower, but is unable to detect any change in it when he looks at it directly.

Often the transition from one colour to the other takes place through a grey which in places is somewhat like the grey of the lower disc, but its unsteadiness makes comparison difficult.

Of the other conditions which affected the combination of the colours the chief were the duration of the observation and eye movements.

The alternating colours in the binocular field were seldom as pure as the uniocular colours. This was partly due to modification of the predominating colour by the inhibited colour, for the saturation of the former was at once increased by closing the eye to which the other was presented. This, however, would not account for the gradual reduction in saturation which was noticed as the observation proceeded. In every case where the subject incidentally examined the uniocular colours at the beginning and the end of an observation he remarked that they were purer at the beginning. Continued observation, therefore, decreased the apparent saturation of the colours, and this must have had precisely the same effect as a reduction in the purity of the stimulus. It was for this reason that at the beginning of the observations, especially in positions (*b*) and (*c*), the colours were purer than at any other time and there was consequently more rivalry: indeed, with colours of low saturation rivalry was detected only at the beginning of (*b*) and (*c*). They were less pure at the beginning of (*a*) than of (*b*) and (*c*) partly because of adaptation produced in (*a*) by the adjustment of the stereoscopic slide and partly because of the after-effects of stimulation in positions (*a*) and (*b*).

Another circumstance which affects binocular colour mixture is eye-movement. Normally we do not keep our eyes still, but let them wander from one object to another, so that we get a large number of retinal pictures of an object from the sensory effects of which our perception of the object is formed. I tried to make the manner of observing as like that of everyday life as possible by directing the subjects not to look steadily at some point, but to let their gaze wander round the edge of the disc and along those portions of the slide which were near it. Generally these instructions were carried out, but sometimes a point was fixated for an appreciable time, and on these occasions rivalry occurred more readily than when the eyes were moving.

One of the most striking results of this series of observations was the demonstration of great individual differences between the subjects. Owing to the difficulty of adjusting the discs to show the required amounts of red, green, etc., the same combinations of colours were observed successively by subjects B. and D., and each took down the other's introspection. This demonstrated beyond dispute the existence of individual differences in the power of combining colours binocularly. With the most saturated colours (360° green in one disc and 257°·1 red in the other) subject B. observed only slow rivalry in which the colours did not seem to be very pure, whereas D. got very rapid rivalry of well saturated colours: at times B. saw the upper disc all red, but only for an instant and at the beginning of an observation, whereas D. saw it changing from red to green and back again a score of times. With less saturated colours there was the same difference between these observers: B. recorded mixture with colours which gave only rivalry to D. Subject J.'s observations were very like D.'s, except that he observed mixture less frequently. What the cause of this difference is I have not discovered.

Another very important feature of this series—one which raises a point not previously recorded in the literature on the subject—was the complete absence of lustre. In this respect, as we shall see later, this series differed from those in which colours of different brightness were combined: either the colours displaced each other in more or less rapid succession, or there was a dull mixture more or less like the uniocular mixture.

(b) *Non-complemenatry colours of the same brightness and saturation, but different hue.*

The colours used in these experiments were of the same or very nearly the same brightness. Table II gives a summary of the results

TABLE II. *Non-complementary colours of the same brightness and saturation.*

Left Blue	Right Red	Subject B.	Subject D.	Subject J.
360°	360°	Vigorous rivalry	Vigorous rivalry	Vigorous rivalry
300°	300°	Rivalry	Keen rivalry	Keen rivalry
240°	240°	Rivalry and mixture. Not alike	Less rivalry: successive and simultaneous	Rivalry
180°	180°	Less rivalry: more mixture; not alike	Less rivalry	Rivalry, successive and simultaneous
120°	120°	Very little change. Mixtures not alike	Slow rivalry	Simultaneous and slow successive rivalry
60°	60°	Unsteady mixture, generally more bluish than uniocular mixture	Rivalry and mixture. Not alike	Rivalry and mixture. Not alike
30°	30°	Mixture. Sometimes alike	Slight simultaneous rivalry and mixture. Never alike	Nearly like uniocular mixture

The background was of the same brightness as the colours, and in focus.

obtained with red and blue. Only the amounts of these colours are shown: the rest of each disc was grey of the same brightness as the colours.

The general results of this series were very like those of the last, and they demonstrate the advantage of having a standard with which to compare the binocular colour. With more than 60° of colour in each disc there was fairly well-marked rivalry; with 60° the binocular colour was generally a steady mixture, but was not like that of the lower disc. It was purple, but thin and undecided, much nearer grey, blue, or red than the colour of the lower disc, which was a quite decided purple. With 30° of colour the two discs were often alike, but sometimes the binocular mixture contained a little more blue or red than the other.

If the antagonism of colours be measured by the degree of binocular rivalry they give, then it is evident from Tables I and II that non-complementary colours are quite as antagonistic as the complementary colours red and green, perhaps more so. The slightly greater antagonism between the reds and blues may, however, be due to the fact that they were not quite equally bright, whereas the reds and greens were.

(c) *Colours of the same hue and brightness but different saturation.*

In these experiments the colour of disc A was pure while the small disc concentric with it was made up of the same colour mixed with varying amounts of grey of the same brightness. The results are summarised in Table III.

These colours gave the same kind of results as were given by those of the same brightness but different hue. As the difference between them decreased, the amount of rivalry in the binocular field became less and less. There was, however, much less rivalry than with colours of different hue. Prolonged observation and movement of the eyes had the same effects as before; the same individual differences appeared, and there was a total absence of lustre.

Over a fairly large range of differences of saturation very great difficulty was experienced in deciding whether there was any change in the binocular image. That there probably were changes akin to those observed with greater differences of saturation is shown by the fact that, over the range referred to, the subjects were very uncertain in their judgments and kept repeating their doubts, whereas with greater or less differences there was little or no uncertainty, there being only unmistakable rivalry or unmistakable similarity between the two discs.

TABLE III. *Colours of the same hue and brightness, but different saturation.*

Left	Right		Subject		
Green	Green	Grey	B.	D.	J.
360°	—	360°	Slight rivalry and mixture	A little rivalry	Rivalry
„	60°	300°	Mixture. Alike	Alike and very slight rivalry	Rivalry and mixture
„	120°	240°	Exactly alike	Exactly alike	Rivalry and mixture
„	180°	180°	„	„	Very slight change
„	240°	120°	„	„	„
„	300°	60°	„	„	Alike
„	330°	30°	„	„	„
Red	Red	Grey			
360°	—	360°	Rivalry	Rivalry	Rivalry
„	60°	300°	Alike and slight rivalry	„	„
„	120°	240°	Less rivalry. Often alike	„	„
„	180°	180°	Alike and doubtful rivalry	Alike and slight rivalry	„
„	240°	120°	Alike and doubtful rivalry	Alike and slight rivalry	Rivalry and a little mixture
„	300°	60°	Alike	Alike	Alike
„	330°	30°	„	„	„

The background was a grey of the same brightness as the colours, and in focus.

(d) *Colours of different brightness.*

Table IV summarises the results of a few typical series of experiments with colours of different brightness. Many combinations were used, but the results of only three are shown. Colours of different hue, complementary and non-complementary, colours of the same hue, and white, black, and greys were used. Those of the same hue differed both in brightness and saturation, for it was not possible to vary the one without varying the other. Brightness was varied by mixing the colours with more or less white or black.

For reasons to be given later the slide used in each experiment was a grey of the same brightness as the lower disc (the uniocular mixture). As this varied from one observation to another, it was necessary to use a series of slides of different brightnesses.

The results of these experiments were in some respects like those already described. Uniocular colours which differed greatly in brightness gave rivalry; those which were only slightly different gave a more or less steady mixture.

TABLE IV. *Colours of different brightness.*

Left	Right		Subject		
Red	White	Red	B.	D.	J.
360°	360°	—	Rivalry	Rivalry	Rivalry
,,	300°	60°	Rivalry and mixture	Rivalry	Rivalry, lustre, and transparence
,,	240°	120°	Rivalry and mixture: never alike	Rivalry and unsteady lustre	Rivalry and transparence
,,	180°	180°	Rivalry and mixture: never alike	Rivalry, lustre and transparence	Lustre and a little rivalry
,,	120°	240°	A little rivalry. Upper lighter	Lustre. Less rivalry	Rivalry, lustre, and unsteady mixture
,,	60°	300°	Nearly alike: lower duller	Nearly alike: slight rivalry and lustre	Rivalry, lustre, and unsteady mixture
,,	30°	330°	Alike	No change: alike or nearly so	Alike or nearly alike
Green	Black	Green			
360°	360°	—	Rivalry and mixture	Rivalry and lustre	Rivalry
,,	300°	60°	Slight rivalry: unsteady mixture	Little rivalry. Lustre. Nearly alike	,,
,,	240°	120°	Slight rivalry and alike	Lustre, slight change and alike	Rivalry and lustre. Upper generally lighter: sometimes alike
,,	180°	180°	Slight rivalry and alike	Less rivalry and lustre	Rivalry and alike
,,	120°	240°	Alike: very slight change	Alike: doubtful change	Alike. Sometimes slight change
,,	60°	300°	Alike	Alike	Alike: upper perhaps more neutral
,,	30°	330°	,,	,,	Alike
White	Blue	White			
360°	360°	—	Rivalry: never alike	Rivalry and brilliant lustre	Rivalry
,,	300°	60°	Slow rivalry	Rivalry and lustre	Rivalry: never alike
,,	240°	120°	,,	,,	Rivalry and a little lustre
,,	180°	180°	Mixture and very slight rivalry. Not alike	Rivalry. At times nearly alike	Rivalry and doubtful lustre
,,	120°	240°	Steady mixture. Upper lighter	Steady mixture, lighter than lower	Rivalry. At times somewhat alike, but less blue in upper
,,	60°	300°	Steady mixture, lighter than lower	Steady mixture, more brownish than lower	Rivalry and mixture
,,	30°	330°	Steady mixture, lighter and more brownish than lower	Nearly alike. Lacked bluish tinge of lower	Sometimes very nearly alike. Upper a little lighter

The background was grey of the same brightness as the lower disc; in focus.

There were, however, some points of difference. The most important were the appearance of lustre and what I can describe only as seeing one colour through the other, or transparence, phenomena which did not appear with colours of the same brightness.

When splashes of each colour were seen in different parts of the disc, they were not so obviously in the same plane. One was like a thin misty film through which the other was seen: both discs seemed to be visible in the same part of the field, but at different distances. The effect was somewhat like that given by a transparent mirror held so that some objects are reflected by it while others are seen through it. It was never steady: it generally occurred at the transition from predominance of one colour to predominance of the other, but was also seen where one colour touched the other; these parts of the field could not, however, be observed carefully on account of the rapidity of the changes. This transparence was quite different from mixture, for each colour seemed to preserve its own individuality, its own hue, saturation and brightness. It was observed by all the subjects.

Although the transparent colour was generally the brighter, this was by no means an invariable rule, for the relation was frequently reversed even with the same observer in the same experiment. I have been unable to discover the conditions that determine which is the transparent colour. At one time I thought it was the colour that filled most of the field, but further investigation has shown that this is not so.

This experience of seeing one colour through another differs somewhat from the visual perception of depth, for the colours are not so perceptibly separated by a distance. It is best described by saying that one colour is seen through the other. It was described by Helmholtz, but his description has since been regarded as a curiosity and otherwise ignored. Dove, who had a reputation among his contemporaries as a careful observer, had previously noticed it and based his theory of lustre on it.

Lustre is another phenomenon given only by colours of different brightness. In these experiments the upper disc sometimes shone like silk, graphite, or a metallic surface. This lustre was of many degrees: sometimes it was brilliant like that of silk or satin seen in a strong light; sometimes it was so dull that it was noticed only by comparison with the duller colour of the lower disc; between these extremes there were many degrees. Brilliant lustre was generally short-lived; it occurred most frequently at the beginning of observations in which dull lustre was seen most of the time. Except with colours which differed only slightly, lustre was unsteady, some parts of the disc being more lustrous than others, some not lustrous at all. When lustre gave place to complete predominance of one colour, the change was most striking; the disc, which previously had been shiny and luminous, became flat and dull. The lustrous character of the upper disc was frequently noticed by comparing it with the other which was always dull, flat, and heavy, as if it had been painted with pigments containing no oil or varnish. Indeed, when the right- and left-eye images were nearly alike, the upper disc differed from the lower only in being brighter, more transparent, or aerial.

Lustre often appeared at the transition from one colour to the other, especially when the change took place slowly. When colours of different brightness were in rivalry, especially if the rivalry was of the simultaneous kind, lustre of the unsteady variety could always be produced by moving the eyes rapidly round the edge of the disc and over the adjoining portions of the slide. Under these conditions with discs of black and white or dark grey and white the effect was like that given by the play of light on the surface of deep, moving, clear water; dark and light effects accompanied and followed each other in rapid succession producing a brilliant and dazzling appearance. On the other hand, lustre could often be destroyed by fixating steadily: it then gave place to successive rivalry.

It is evident from a comparison of the results of this and the previous series of experiments that binocular lustre can be produced only by combining colours or greys of different brightness.

As this conclusion is directly opposed to the opinion of many writers on the subject, it may be advisable here to recall the only attempts that have been made experimentally to decide the question.

These were made by Dove (17) and were held by him to prove that lustre could be produced by combining colours alike in brightness. By means of a stereoscope he superposed squares of blue and yellow paper and found that the lustre given by them was more vivid when they were seen through a blue glass than when this was removed. As this glass greatly reduced the difference between the brightnesses of the colours, but increased the brilliance of the lustre, he concluded that difference of brightness could not be essential to the production of lustre. I have repeated this experiment, but have been unable to corroborate Dove's results. Even if they be accepted, the only conclusion that can be drawn from them is that lustre is given more readily with certain moderate brightness-differences than with certain other larger differences.

In the other experiment he held coloured glasses before the eyes and darkened each uniocular field in turn by moving first one then the other glass so that the rays of light entering the eye had to pass through glass of gradually increasing thickness: under these conditions Dove held that lustre was given continuously even when the uniocular fields were equally bright. This observation also I have repeated without being able to confirm Dove's result: I find it impossible to detect lustre on looking through grey glasses, nor do I detect it with coloured glasses, if each darkens every part of the field equally. These two observations, both unverified, can have little weight against the results of experiments in which colours of carefully adjusted brightness are superposed.

Lustre was recorded more frequently in the black-white series (not shown in the table) than in those in which colours of different hue and brightness were combined, and these gave rivalry more often than did greys of the same brightness as these colours. Evidently, therefore, the addition of differences of hue to differences of brightness makes it more difficult to keep the colours simultaneously in the binocular field.

It will be seen by comparing the summaries of Table IV which gives a few specimens of the results of some dozens of series of experiments, that there is a little difference in the extent to which different observers get binocular lustre. Subject B. recorded it less frequently than the other subjects.

In these experiments the differences between binocular and uniocular mixtures were more noticeable than in those in which colours of the same brightness were combined. Except with hardly perceptible differences in the brightness of the uniocular images, the two mixtures were clearly different: while the uniocular mixture was dull, uniform, and steady, the other was either bright, unsteady, or not uniform.

Especially interesting in this respect are the results of combining a white disc with one in which white was mixed with varying amounts of some colour. Even with the smallest differences used, viz. 30° of colour, all the subjects detected a difference between the two mixtures. The colour of the uniocular mixture was always more definite than that of the other. This difference was not always described in the same way. Sometimes it was said that the colour of the binocular mixture was more complex than the other, sometimes that it was not such a hard, heavy colour, that it was lighter, or even of a slightly different hue, *e.g.* brownish as compared with the blue colour of the other. The difference was more easily detected than described: at times it was quite indescribable. This corroborates the results of the observations of Helmholtz [(23), 776], Rood (40-42), and Trendelenburg (49), all of whom noted differences between binocular and uniocular mixtures.

4. EFFECT OF BACKGROUND.

(a) *Brightness of the background.*

In the experiments which have just been described the colours were surrounded by a grey background the brightness of which was equal to that of the lower disc. We have now to show why this background was used and to demonstrate the effect of the brightness of the background on binocular combination.

For simplicity of exposition let us consider first the mixture of grey and white. When surfaces of different brightness are placed side by side, the lighter becomes still lighter and the darker still darker, the amount of brightening or darkening varying directly with the difference between the brightnesses of the surfaces. On this account the brightness of the discs used in the above experiments varied with that of the background: with a dark slide (one of black velvet paper) they were much lighter and more aerial than with a grey of medium brightness: with a white slide they were dark, heavy, and lacked entirely the atmospheric appearance that was given by the black slide. This, however, was not the only effect of the brightness of the background.

If we suppose that the amount of darkening or brightening due to contrast varies directly with the amount of difference between the contrasting surfaces, then the only effect of the background should be to change the brightness of the resultant: it should not affect the power of combining the discs binocularly, for the darkening of one grey and the brightening of the other by contrast with the background will be such that the uniocular images will differ by the same amount and the mean of their brightnesses will be increased or decreased by the same amount as the other mixture. This, however, is not what we find. A black slide, as compared with one of a grey of medium brightness, has the same effect as a reduction of the difference between the brightnesses of the greys that are combined binocularly: it reduces the amount of rivalry between them and produces fairly steady lustre and mixture with greys which are to some extent in rivalry when a slide of medium grey is used. This effect is not very great, but, though I have not attempted to get a measure of it, the following extracts from the records will show that it is quite appreciable:

Subject J. Left upper disc, 360° white. Right upper, 180° black and 180° white. Lower, 90° black and 270° white.

Slide: deep black, in focus.

(*a*, *b*, *c*). "Two discs indistinguishable from each other. Very bright soft grey. The lower is perhaps slightly darker than the upper."

Slide: grey of the same brightness as the lower disc, in focus.

(*a*, *b*, *c*). "In the upper disc there is rivalry between a darker and a lighter disc. I think there is sometimes a little lustre present. There is at times a transparent effect in (*a*), (*b*), and (*c*): I see one brightness through the other. I think the upper disc is darker than in the previous observation (*i.e.* with the dark slide). Rivalry which is present here was absent in the other. The transparent effect was not present in the first observation."

Slide: white, in focus.

(*a*, *b*, *c*). "There is rivalry here between a dark disc and a lighter one, but not so much as in the last observation. Sometimes a large part of the dark disc brightens. The lower disc here is, I think, darker than in the last observation (with the grey slide). I think the dark rivals are darker than in the last observation. I did not detect lustre."

A white background does not affect the mixing of the colours so much as a black one; it makes the colours look much darker and heavier, and seems to have some, but very little, effect on their mixing. The effect of a black background in apparently reducing the difference of intensities of lights stimulating corresponding parts of the retinae is like its effect in reducing the difference between two greys stimulating adjoining parts of the retina (11).

The effect of the brightness of the background is quite as striking when colours, as blue, red, green, etc. are combined. A black ground not only increases their brightness, but reduces their saturation and makes them combine more easily: the colours look more aerial, more transparent than with a lighter background. With white they are darker, heavier, and more solid than with a grey ground, and they combine a little more easily, but not so readily as with a black ground.

When the colours differ greatly in brightness and the background is black, the lighter colour generally occupies the field longer than the other, and any mixture there may be is usually more like the lighter colour than the other. With a white background it is just the reverse; the darker colour tends to predominate.

As a background of grey of the same brightness as the colour of the uniocular mixture alters the saturation and brightness of the combining colours less than any other, and its effect on these colours is therefore as constant as possible, this background was used in the experiments already described.

(b) *Focusing of the background.*

Hering [(25), p. 595] attributed failure to get binocular colour-mixture partly to placing the colours in focus in a stereoscope. He stated that mixture was produced most readily by throwing the slide out of focus, for thus contours were obliterated, and their obliteration, even if they were similar, favoured mixture. Rivers [(39), p. 277] found that he could see binocular mixture better with myopic or hypermetropic vision, and attributed this to blurring of outlines. Most experimenters who have tried to mix pigment colours binocularly have used small squares of coloured paper pasted on a sheet of cardboard seen through the lenses of a stereoscope. It is very difficult to cut such squares so that they are of exactly the same size, and to paste them accurately on cardboard, and it is possible, as Hering says, that by throwing the slide out of focus these small differences of form are obliterated and mixture facilitated thereby. Further, when the colours are in focus, the grain of the paper can be detected, and this may help to promote rivalry.

In my experiments, however, no advantage has followed from putting the slide out of focus; in fact, this rather hindered mixture. The difference between the effects obtained with the slide in focus and out of focus is not very great, but all the records show that where there is a difference, the putting of the slide in focus favours mixture. Rivalry is less complete and sometimes gives place to fairly steady lustre or mixture when the slide is changed from the out-of-focus to the in-focus position. The difference is not so great as that produced by using a black slide instead of a grey one; sometimes it is not noticeable at all; but it is always in the same direction.

The only reasons I can suggest for getting a result so different from that of Hering and Rivers are the complete elimination of dissimilarities of contour by using circular patches of colour of exactly the same shape and size, and the elimination of irregularities in the colours by using rotating discs. Probably the latter is the more important condition.

Dissimilarities in the shapes of the combining patches of colour favour rivalry when these are in focus, whereas throwing them out of focus blurs their edges and removes this cause of rivalry; but when these dissimilarities are absent and the grain of the paper cannot be detected, focusing favours mixture because of the similar contours.

One objection to putting the slide out of focus is the difficulty of maintaining steady convergence. Another is the painful, distressing nature of the observations when they are continued for some time. For some reason the subjects were sometimes unable to describe what they had seen under these circumstances. One is tempted to say that, if a subject does not know which of the two colours he is seeing, he must be seeing a mixture; but such reasoning is fallacious, for a mixture is just as definite and recognisable as either of the colours that are to be combined, and therefore if the subject cannot say what he has seen, it will be as incorrect to say that he has seen a mixture as that he has seen either of the uniocular colours.

Over two-thirds of the observations described in the preceding pages were made with the slide both in focus and out of focus, but in the end the out-of-focus procedure was abandoned.

5. EFFECT OF SIZE OF COLOURED SURFACES.

In order to find whether mixture was affected by the sizes of the coloured areas several grey slides were used; in one the holes were 26 mm. in diameter, in the others they were 21, 13, 6·3 and 3 mm. respectively. These were held in turn before the same set of colours, and the subjects described the binocular resultant, and then compared the effects obtained with the different slides.

With all the slides except the one in which the holes were 3 mm. in diameter, the results were very nearly or exactly the same. There may have been just a little more rivalry with the larger holes than with the smaller; but the evidence is not conclusive. The results of the observations with the 3 mm. holes were somewhat contradictory. With colours of the same brightness this slide generally gave less frequent and less pronounced rivalry than the others, but with colours of different brightness the changes were quite as frequent and even more pronounced; thus with pure red and pure blue the alternation was not so frequent with the very small holes, but with black and white it was much more frequent. Though this seems to be the general tendency of the results, they are so often contradictory that I do not attach much importance to them.

The contradictory character of the results is probably in part due to the difficulty of avoiding fixation when the discs of colour are very small, and the absence of this difficulty in the case of the larger discs; for, when a point on a large disc is fixated, there is just as much alternation as with the smaller discs. It may also be partly due to the greater ease with which the colour of a large area can be detected: often changes were observed in the smaller disc, but could not be described, *i.e.* the subject did not know in which direction the change was taking place.

These results are quite incomparable with those of Trendelenburg, who succeeded in mixing spectral colours, for he experimented with very much smaller areas of colour—areas subtending an angle of not more than thirty minutes at the eye.

6. INFLUENCE OF CONTOURS.

In the experiments which have just been described the uniocular images have been alike in form and partially different in colour. We have now to consider the effect of contours on the mixing of colours. By contours are meant any variations of brightness, hue, or saturation that introduce form into the visual field.

It is generally agreed that dissimilarities of contours hinder binocular mixture, and experiments with postage stamps have raised a presumption that colours which would not mix otherwise can be made to do so by introducing into them similar complex contours.

Absolute similarity of contours in the two retinal images is unattainable: even in the experiments which have just been described it is only the central parts of the fields that are similar in this respect; their peripheral parts bounded by the partition of the stereoscope and by the slide-holder are different, but it is doubtful whether these dissimilarities play any great part in hindering or facilitating mixture. Apart, however, from differences in the periphery of the fields, there are various degrees of similarity of contours which may have different effects on binocular mixture. I propose to consider a few extreme cases—to find the effect of dissimilarity of shapes and sizes of the coloured areas, and the effect of dissimilarity and complete and partial similarity of contours within equal and similar areas.

(a) *Dissimilar contours.*

Two kinds of dissimilarity in the forms of the coloured areas must be distinguished, for they have different effects on the binocular combination of colours: distinction must be made between areas bounded by contours which in the binocular field nowhere stimulate corresponding points, and areas bounded by contours which in some places do stimulate corresponding points.

The following method of finding the effect of dissimilar and entirely non-corresponding contours is the one I have used. It is simple and has the merit of lending itself to use with rotating discs. A slide similar to that described on page 515 was made of grey paper, but one of the upper holes was made larger than the other. When this slide was placed in the stereoscope and through it were observed the colours of the rotating discs, the two lower holes coincided, while the small upper one lay within the larger. The use of this slide alternately with one in which all the holes were alike in size gave a fairly delicate method of studying the effect of dissimilarity of contours. Several of these slides were made with holes varying in diameter from three to twenty-six millimetres. The method of observation was exactly the same as that adopted in finding the effect of saturation, brightness, etc., on mixture.

In the binocular field there was always predominance of the uniocular colours in the neighbourhood of the edges of the discs. When the small disc was red, the large one blue, and the slide grey, a small red disc was seen surrounded by a ring of grey which gradually shaded off into a pure blue at its outer edge. When the ring surrounding the central disc was fairly broad, its colour changed slightly, sometimes the grey encroached on the blue, sometimes it lost ground: when the ring was narrow, the centre of the smaller disc was occasionally tinged with the colour of the larger. Otherwise, the whole impression was fairly steady. Only by closing and opening one eye or by fixating very steadily could any very marked change be produced: the whole of the large disc then became blue for an instant either with or without a central circle marking the position of the other disc, or this gave place to the small disc surrounded only by the grey of the slide. Normally there was little or no change. The colour of the central disc was generally as pure as that of the uniocular image; so also was that of the outermost part of the large disc.

This predominance of the uniocular colours about the contours was given with all degrees of difference of coloration. It was very different from the experience given by combining areas equal in size. In the latter case with moderate colour differences there was more or less rapid rivalry, and with smaller differences either lustre or a mixture intermediate in tone between the combined colours.

The effect of combining coloured areas bounded by dissimilar contours which in places stimulated corresponding points was demonstrated in the following manner. In a piece of grey paper I cut a vertical and a horizontal rectangular slit, each 33 mm. by 6 mm., in such positions that when they were combined in the stereoscope they crossed at their central parts. Below these were two small holes 13 mm. in diameter and the interocular distance apart, while to the right of and above the centres of the rectangular slits were similar holes the same distance apart. This slide was held so that through the lowest pair of holes was seen the colour of the lower disc (the uniocular mixture); through one of the upper holes and the rectangle adjacent to it was seen one of the colours to be combined, while through the other slit and hole was seen the other.

When this slide was placed in the stereoscope two rectangles of different colours were seen overlapping so as to form a cross with one disc below and another in the upper right-hand angle of the cross but not touching it; the colour of the lower disc was the uniocular mixture of the colours of the rectangles, while that of the upper disc was the binocular mixture of the same colours. It was thus possible to compare the colours of the various parts of the rectangles with both the binocular and the uniocular mixture of equal discs of the same colours, a comparison which could not readily be made in the old form of the experiment in which strips of coloured paper were pasted on a piece of cardboard.

The following are specimens of the records of experiments made with this slide:

Subject D. Left, 300° blue and 60° grey. Right, 300° yellow and 60° grey.

"In the upper disc (binocular mixture) there was lustre; in the lower, dull mixture. The rectangles kept alternating rapidly. The whole of either the vertical or the horizontal rectangle stood out on a grey background on which were the ends of the other: the ends of the inhibited rectangle stood out clearly, but towards its centre they merged gradually into the grey background. Where the rectangles crossed, there was never a colour like that of either of the discs. Only once or twice did I get a suspicion of lustre there, but it was only momentary and not sufficiently pronounced to make me certain that it was lustre.

Subject D. Left, 180° red and 180° grey. Right, 180° blue and 180° grey. These colours were of about the same brightness.

The colour of the upper disc was not like that of the lower nor was it like either of the uniocular colours: it kept changing, but very slowly.

Sometimes one rectangle appeared with only the ends of the other. Sometimes both were seen: when this happened, the central square was very similar in colour to the lower disc, but it was difficult to compare them on account of the changes that were always taking place. When this square did appear, it was surrounded on all sides by a grey border which merged gradually into the colours of the rectangles. This effect was never steady."

Subject D. Left, 60° red and 300° grey. Right, 60° blue and 300° grey. These colours were of about the same brightness.

(*a*) "Neither of the rectangles disappeared: the edges of both were present all the time. The colour of the central square was the same as that of the discs. The discs were alike. The central square was surrounded by a grey halo which faded gradually into the colours of the limbs of the cross.

(*b*) (Colours interchanged). The rectangles predominated alternately. Towards the end of the observation the central square appeared with a grey halo separating it from the limbs of the cross.

(*c*) This was like observation (*a*), but once while fixating I got predominance of the blue rectangle.

Once or twice in (*a*) the vertical rectangle became all grey except at its edges where it retained its colour."

In every observation the rectangles displaced each other with greater or less rapidity; at one time there were seen the complete horizontal rectangle and the ends of the other separated from it by an area of grey; at another time it was the vertical rectangle which predominated. The pure uniocular colours were seen in the predominating rectangle and at the sides of the visible parts of the other. Sometimes, when the colours were not very different from their grey background, it was only at the sides of the rectangles that the pure uniocular colours were seen; over the rest of the area there was a varying mixture with grey.

This effect was never steady. With colours which differed greatly in hue, saturation, or brightness the alternation was very rapid; with smaller differences it was much less so. Frequently the entire edges of both rectangles were seen simultaneously forming a cross, the central square of which was of the same colour as the binocular mixture in the upper disc: this square was surrounded by a grey halo which merged gradually into the colours of the limbs of the cross which at their edges were seen in the pure uniocular colours. This effect was seldom given by colours which differed greatly, and it never lasted more than a second or two. It was given most frequently with colours which were nearly alike. It was easily produced by rapidly opening and closing the eyes.

Binocular mixture was produced much more frequently in the coloured discs (*i.e.* with exactly corresponding areas of colour) than in the square where the rectangles crossed: indeed, it was very rarely that the colour of the square differed from both of the uniocular colours. It is possible that the great amount of alternation observed in these experiments was due partly to the definiteness of the contours separating the coloured areas from the background. With slides of paper on which rectangles were painted in very faint washes of water-colour I got much less rivalry than in these experiments; the central square stood out clearly for a considerable time surrounded by a grey halo. It is clear that these differences of form tend to keep the uniocular images apart and lead to keen rivalry of their corresponding parts.

To find the effect of dissimilar contours within corresponding coloured areas of the same size and shape, the slides of grey paper described on page 516 were used, but across each of the upper holes five parallel strands of black silk were fixed so that in the binocular field they were at right angles. As one would expect, there was keen rivalry between the black threads, and this rivalry affected the colour combination, as will be seen in the following extracts from the experimental records:

Subject D. Left, 300° red and 60° grey. Right, 360° red.

(*a*) "Continual rivalry of lines: often both sets seemed to be present simultaneously: never was one set there alone. The colour of the upper disc did not change: it was not perceptibly different from that of the lower (uniocular mixture): it may have been slightly purer, but of this I am not certain.

(*b*) (Colour interchanged). Each set of lines at first appeared in succession bringing with it the colour of its own disc, so that there was very clear successive colour rivalry. After a time there was simultaneous rivalry of the lines, *i.e.* some parts of the disc were filled with portions of one set and other parts with portions of the other set, and the colour of the disc did not then change perceptibly.

(*c*) As in (*b*), but successive contour and colour rivalry did not last long. It quickly settled down to the form it assumed in (*a*): the colour was then slightly purer than that of the lower disc, but very nearly like it.

With uncontoured discs the colours of the upper and lower were exactly alike."

Subject D. Left, 60° red and 300° grey. Right, 60° blue and 300° grey.

(*a*) "At first and for a long time the colour of the upper disc was just a little more saturated than that of the lower. At the end of the observation there was a little change from one colour to the other, but very little.

(*b*) Very clear successive rivalry of both colours and lines. It became a little less complete, and a little simultaneous rivalry appeared.

(*c*) Successive rivalry at first. This passed into simultaneous rivalry in which the uniocular colours sometimes appeared only between pairs of lines. At the end of the observation the colour of the upper disc was nearly like that of the lower, but not so much so as in (*a*).

When the observation was repeated in order to find whether the colour always changed when the contours changed, I got both successive and simultaneous colour rivalry."

The similarity in the behaviour of dissimilar contours and dissimilar colours is worthy of note, for it suggests the necessity of explaining them in the same way. As with colours so with contours, rivalry may be successive or simultaneous, *i.e.* each image may predominate in turn, entirely excluding the other, or portions of each may be seen simultaneously in different parts of the binocular field.

At the beginning of an observation, especially in positions (*b*) and (*c*) (*i.e.* after interchanging the right- and left-eye images), and with colours differing greatly in hue, saturation or brightness, rivalry was almost invariably successive: the uniocular colours then appeared in their original purity. This successive rivalry of contours was detected at some time in practically all the observations: when it occurred, the slightest difference of coloration of the two images could be detected.

Simultaneous rivalry of contours occurred most frequently with colours nearly alike and at the end of the observations, especially in positions (*a*) and (*c*). Sometimes it seemed as if both complete sets of lines were seen simultaneously forming a kind of network, but, as this impression was only momentary and disappeared as soon as the subject began to examine the field carefully, the observations had to be repeated several times before all doubt as to its occurrence was removed. When the eyes were opened and closed rapidly, both sets of lines were seen simultaneously forming a network of squares. Generally, however, there was just a changing medley of lines. In this kind of rivalry the colour of the binocular image was often like that of the uniocular mixture: it was most markedly so when the rivalry of contours was such that the subject did not see anywhere a large part of the contours of one image. The colour of the binocular image depended entirely on the nature of the rivalry of contours: if large portions of the lines of either image appeared in the binocular field, then between them was seen the colour of the corresponding uniocular image, even though the difference between the uniocular colours was just discernible; but if the changes occurred so rapidly that the subject did not hold any pair of lines of either image for an appreciable interval, the colour was more or less like that given by discs without contours.

The attitude of the subject seemed to some extent to determine his impressions. Often in comparing the colours of the two discs the subject was most acutely aware of the colours of the two areas and only dimly aware of the changes that were taking place in the lines of the upper one, but, when he tried to find how much of the lines of each image appeared and whether they brought with them the colour in their immediate neighbourhood, the whole effect changed and he detected patches of uniocular colour between pairs of lines belonging to the same image, patches of colour which previously had not been detected.

On comparing these results with those of the experiments in which uncontoured areas were used, it is obvious that dissimilarity of contours greatly hinders binocular colour mixture, but does not always prevent it.

(b) *Similar contours.*

A very simple method of mixing colours in which are exactly similar contours is that of Stirling: it consists in combining in the stereoscope postage stamps of different colours printed from the same die. I have repeated Stirling's experiment (48) with a set of cards mounted with pairs of such stamps (kindly lent by Dr Watt), and find that, though they frequently give rivalry, yet mixture does seem to occur more readily than with uncontoured surfaces, and that the mixture appears to be that which would result from mixing the colours on the same part of one retina; but, in the absence of any means of comparing these mixtures directly, it would be unwise to draw from these observations any conclusions as to the effect of similar contours in facilitating binocular mixture. In experimenting with these postage stamps the following curious facts appeared. It happened that some of the stamps were alike in design, but differed in the words and figures expressing their values. When they were combined in the stereoscope, these words appeared alternately and were often seen, not in their own colours, but in a mixed colour like that of the rest of the stamp. The genuineness of this effect was clearly demonstrated by closing the eye before which was the stamp of the other colour, when the words appeared in their true uniocular colour. Thus the colour of the predominating contour was modified by the colour seen by the other eye, even though the contours of the latter were not seen.

From a red penny and a green halfpenny King George postage stamp (issue 1912) I cut out the central head-pieces and, after pasting them on a piece of white cardboard, combined them in the stereoscope. Sometimes the binocular image was green, sometimes it was red, but generally it was a puzzling grey. These stamps are printed from different dies: the bust on the red one is distinctly larger than that on the green, and yet they give *one* grey image in which the uniocular pictures cannot be separately detected. This seems to indicate that the mixture which is given so readily by postage stamps may not be due so much to the exact similarity of their contours as to the unsaturated character of the colours.

I have endeavoured to modify this stamp experiment in such a way as to make it possible to compare the binocular mixture of the colours of similarly contoured surfaces with that of the colours of uncontoured surfaces, and each with the corresponding uniocular mixture. After unsuccessfully trying to devise some satisfactory method of using the colours of rotating discs, I was at length reduced to using printed drawings cut from books stained with different colours and coloured reproductions of a photograph printed from a half-tone photo-process block, such as is used for reproducing photographs in the daily newspapers.

Coloured prints from the engraved process block seemed to give mixture even more readily than postage stamps, but comparison colours could not easily be found because the prints were made up of tiny dots of colour the varying sizes of which produced the necessary variations of brightness and saturation in the different parts of the field.

More satisfactory were the stained drawings. From the front page of some old numbers of *Punch* I cut out the head of Toby and stained the prints by immersing them in solutions of stains of various kinds—methylene blue, picric acid, eosin, carmine, methylene green, etc. Painting the prints was found to be quite unsatisfactory, because it was impossible to spread the colour evenly over the paper. The pictures were cut out by means of a circular wad-punch, and were 26 mm. in diameter. They were pasted on pieces of grey paper so that, when seen through the lenses of a stereoscope, they coincided.

The prints on each slide were of different colours. Below these were two discs of blank paper which were like them in both size and colour. Between the upper and lower discs and a little to the right of them two circular holes 13 mm. in diameter were pierced in the grey paper. Two large discs 120 mm. or more in diameter were stained in the same way as the coloured pictures: these were interleaved so that they formed a disc half of which was in one colour, half in the other. When this was rotated, it gave the uniocular mixture of the colours of the prints: by holding the slide in a suitable position, this colour was seen through each of the small holes.

When one of these slides was placed in the stereoscope and held in front of the proper rotating disc, there were seen three coloured discs on a background of grey: the upper was formed by combining two coloured prints, the lower by combining two uniform discs of the same colours as these, while the colour of the middle disc was given by rotating these colours in equal proportions. Thus it was possible to compare the binocular combination of colours in a contoured surface with their combination in an uncontoured surface, and each of these with the uniocular mixture of the same colours.

Several combinations were used, *e.g.* white and several shades of blue (methylene blue), green (methylene blue and picric acid) and carmine, blue-green (methylene green) and carmine, yellow (picric acid) and blue (methylene blue), etc. Some pairs of colours differed in brightness, others were equally bright: hence it was possible to find whether differences of brightness in contoured areas had the same effect as in uncontoured areas.

It is unfortunate that the colours used in these experiments cannot be standardised. They suffice, however, to demonstrate the effect of similar contours on binocular colour mixture.

The colours were reversed by turning the slide upside down. Otherwise the procedure was exactly the same as in the experiments with uncontoured discs.

The following are specimens of the records of these experiments:

Subject D. Left, light brick red (like the colour of a penny postage stamp). Right, blue (about equally bright).

(*a*) "The colour of the contoured area was steadier than that of the other, but it changed slightly. It was not like the uniocular mixture: it was more clearly blue and red. In some places the red was stronger, in others the blue. The lower disc was alternately red and blue.

(*b*) Like (*a*), but the difference between the uniocular and binocular mixtures was still greater. For a long time both binocular discs were a very pronounced red, almost as red as the right-eye image; then the colour of the contoured area became more like the uniocular mixture. There was rivalry in each, but more in the uncontoured disc.

(*c*) Like (*a*), but more rivalry in both discs.

At no time was either of the binocular mixtures like the uniocular."

Subject D. Left, green. Right, carmine, as bright as the green.

(*a*) "Rivalry in both discs, but more in the uncontoured one. The colour of the other rapidly became a neutral grey very like the grey of the background, and not like the dull brownish red of the uniocular disc.

(*b*) More rivalry than in (*a*): it was at first successive, then simultaneous. The rival colours were almost as pure as the uniocular colours.

(*c*) More rivalry than in (*a*), but less than in (*b*). The contoured area was never like the uniocular mixture, but there was much less rivalry in it than in the other. The uniocular mixture was a dull almost brownish colour, while the binocular colour was reddish or greenish or greyish."

Subject D. Left, blue-green. Right, carmine of the same brightness.

(*a*) "There was a great difference between the upper and lower (uncontoured) discs. The colour of the former was very steady, while that of the latter was always changing. Throughout the observation the colour of the contoured area was nearly like that of the uniocular mixture: several times a large part of it was of the same colour. The lower was either carmine or blue-green.

(*b*) Both were either nearly pure blue-green or nearly pure carmine: most of the time they were blue-green. They were never like the very dull greyish purple of the uniocular mixture.

(*c*) Like (*a*), but there was a little more rivalry."

Subject D. Left, white. Right, blue (about as dark as the background).

(*a*) "The contoured disc was very like the uniocular, but not quite so uniformly coloured. In the other there was a little rivalry: blue and white were seen in different parts of the disc: it was clearly lustrous and very different from the uniocular mixture, which was flat, dull, and uniform.

(*b*) At first both discs were white: then the blue of the other image appeared. These alternated for some time. Later the contoured disc became steadier, but it was never so steady as in observation (*a*), and it generally differed from the uniocular mixture.

(*c*) Like (*a*)."

While the presence of similar contours does not prevent binocular colour rivalry, it does to some extent make it less rapid and less complete. With colours which differed greatly in brightness or hue, both successive and simultaneous rivalry were observed in the binocular discs, but in the contoured disc successive rivalry did not last so long, and the alternating colours were not so pure as in the other. With smaller differences of hue and brightness the contoured disc was appreciably different from the other: sometimes its colour was a steady mixture, while that of the other was clearly changing.

The difference between the contoured and the uncontoured areas was indicated in a variety of ways. Often it was said that the former contained less of either of the uniocular colours than did the latter, that these colours were less easily seen in it, and that the patches of uniocular colour that did appear were not so large and not so pure. Frequently while some kind of change was observable, distinct patches of uniocular colour could not be detected in it as in the other. There was a tendency for the whole of the contoured area to appear in a mixed colour which inclined more or less as a whole now towards one, now towards the other uniocular colour. When an observation opened with rivalry in both binocular discs which later became less vigorous, it invariably ceased or became less acute first in the contoured area, and the colour of this area always resembled the uniocular mixture more than did that of the other disc.

If one of the uniocular colours was very dark and the other very light, there was a tendency for the lighter colour to predominate in the binocular field. This tendency was strongest in the contoured area and at the beginning of an observation, especially after interchanging the pictures presented to the eyes. Sometimes the lighter print would remain unchanged for thirty seconds, but usually a tinge of the other colour appeared in it. In a slide showing a white and a blue disc, the latter being so dark that the contours on it were scarcely visible, the deep blue of the contoured disc was hardly ever seen, but the white contoured area stood out clearly either pure or with a slight tinge of blue.

In order to study this effect more closely I used a series of eight slides on each of which were two white and two blue discs. The latter had been stained with methylene blue to different degrees of coloration. The brightest were a very pale sky blue; the darkest a very deep blue, so deep that its contours could not be distinguished at a distance of a couple of yards.

These slides gave some remarkable results which throw considerable light on Fechner's paradoxical effect. In both binocular mixtures and with all shades of blue except the lightest there was more or less change of coloration, the amount varying with the subjects and the difference between the uniocular images: very rarely was either exactly like the corresponding uniocular mixture.

The lightest blue gave a binocular colour which was clearly a mixture of the white of the one image and the blue of the other. In the contoured disc the colour was to most subjects quite steady, and in the other there was little or no change. The binocular mixtures were seldom exactly like the uniocular: even when no rivalry could be detected in them, they were bluish or whitish while the other was in comparison greyish. The colour which most resembled the uniocular mixture was always that of the contoured disc.

The depth of coloration of the binocular image increased steadily with the depth of the blue of the prints until a point was reached beyond which there was no more deepening of coloration. When this point was passed, the depth of the binocular blue remained fairly constant for a while, after which the image became whiter, till with a very deep blue the figure on the contoured disc looked white with scarcely a touch of blue in it. The edges of this disc were often dark blue shading off rapidly into white, so that it seemed as if the white figure was surrounded by a halo. Though white, it was not quite like the uniocular white: sometimes the print seemed to be seen through a layer of transparent blue fluid: it seemed as if besides the pure white there was something else present which appeared about the contours and not in the spaces between them.

This was very different from what was seen in the uncontoured disc. The colour of the latter was seldom steady: sometimes the uniocular colours followed each other in succession or were seen simultaneously in different parts of the field, but generally there was an unsteady and brilliant lustre. Occasionally one colour seemed to be seen through the other. Even when there was rivalry in both discs, the rival colours were much purer in the uncontoured disc.

An examination of the blue discs shows that it is probably to their contours that these effects are due, for, as the coloration becomes deeper, the contours become less and less prominent. The contours on a blue disc, which with a white one gives a white binocular image, are not easily distinguished; they are so indistinct that in the struggle of the two fields they appear to be powerless against the strongly marked contours of the other image.

For this view there is much evidence in the records of these experiments. There is an almost indescribable complexity about the contours of the image formed by combining a white with a moderately dark print: there is a suspicion of blueness about the contours that is absent from the rest of the picture. This effect seems to wax and wane, as if the indistinct contours of the dark blue disc fluctuate in much the same way as a very faint shadow fluctuates when it is fixated. When a white contoured area is combined with a similar but uncontoured blue area, the effect is not the same; here a white disc is seen either alone or through a very thin film of blue, the blue being spread fairly evenly over the whole surface.

Again, if the contours be equally clear and the colours equally saturated and equally bright, there is a violent struggle between the two images, each apparently being as strong as the other, or a binocular mixture which inclines now towards the one colour now towards the other (see pp. 536–7).

Additional evidence is given by the observations of those whose optical defects have not been perfectly corrected. One subject could with the left eye see the pictures in the stereoscope more clearly at a shorter distance than with the other, and he found that with the slide in one position there was a tendency for the left-eye picture to predominate whereas when it was farther away the other picture appeared more frequently.

It happened that on one slide on which were blue-green and carmine discs, the outlines on the blue-green were more clearly printed than on the other, and that, probably on this account, the binocular mixture of the colours on the contoured disc was always more blue-green than either the uniocular or the other binocular mixture.

If the white contoured disc of a blue and white slide, which gave a white binocular image, was covered by a disc without contours, the dark blue image with its indistinct outlines could be seen quite easily. Similar results were given by blue and white discs on a white background.

When the figure on the darkest blue disc was outlined by being painted over with white pigment, there was vigorous rivalry between the two pictures with some unsteady but brilliant lustre. All these facts are readily explicable on the assumption that the colour of the binocular image depends in a great measure on the relative prominence of the contours in each, and are not easily explained in any other way.

While the colour of each binocular image is almost invariably clearly different from that of either of the combined images and is a compound of both, yet it is very seldom exactly like the uniocular mixture, at any rate with the range of colours used in these experiments, which was necessarily somewhat limited. Even when it is apparently unchanging, it differs from the uniocular mixture and inclines towards one of the uniocular colours. Only on very rare occasions with colours equally bright and contours equally prominent and after somewhat prolonged observation is a binocular mixture obtained which is like the uniocular. It does not last long, and is detected where contours are most plentiful. Sometimes both colours are clearly present, but not in the same way as in the uniocular mixture: the latter is a uniformly mixed colour whereas in the binocular mixture the colours seem to be somehow separated. This curious effect may be partly due to slight irregularities in the surface of the paper or in the prints, but, as it was detected also in the earlier experiments in which the colours of rotating discs were mixed, this can be only a partial explanation.

A striking feature of these observations is the difference between the two binocular images in respect of the amount of lustre and transparency in them: while the uncontoured area was often markedly lustrous, the other was generally dull; on the rare occasions on which lustre was reported in the print, the subject showed some hesitation, as if he thought he was not using the correct word to describe what he saw. The only slide which gave a clearly lustrous effect in both discs was one in which one picture was drawn in black lines on a white ground and the other in white lines on a black ground. This slide gave vigorous rivalry, and at times, when one image was giving place to the other and the whole area was filled with a confused medley of lines and colours, there was a flash of almost dazzling lustre.

Though very little lustre appeared in the binocular contoured area, yet, as has already been remarked, when the combined colours were of different brightness, there was a perceptible difference between this and the uniocular mixture: while the latter was dull and flat, the former seemed to be seen through a layer of transparent fluid; it looked brighter, but not lustrous. This effect was never given by colours of equal or nearly equal brightness and was probably due to slight irregularities in the two surfaces caused by the grain of the paper.

It is probable that this is what some of the subjects meant when they said that the binocular mixture of the contoured disc was more shiny or more transparent than the other. When one colour was very dark and the other very light, the mixture of the two about the contours and the predominance of the lighter colour in other places gave the impression of seeing this colour through something else, which, however, was not clearly the dark uniocular disc. The significance of the absence of lustre from these contoured areas will be considered later in discussing the theory of lustre.

(c) *Stereoscopically similar contours.*

The effect of stereoscopically similar contours on binocular colour mixture may be studied by means of coloured stereoscopic slides or coloured glasses. Both methods have their advantages and their limitations. One advantage of the first method is the opportunity it gives of varying the coloration indefinitely. The following simple form of it is the one I have used.

Series of slides were made like that shown in Fig. 2 by pasting triangles of coloured paper in the triangles marked *A*, *B*, *C*, *D*, *a*, *b*, *c*, *d*. The coloured papers were those used in the experiments already reported. They were pasted on sheets of white cardboard, and black lines were drawn along their edges. Many colours were used. The following is a typical series of slides:

1. Both squares all green.
2. The whole of one square green, the other blue.
3. *C*, *B*, *c*, and *b* green, and *A*, *D*, *a*, *d* blue.

4. *A*, *C*, *a*, *c* green, and *B*, *D*, *b*, *d* blue.
5. *B*, *D*, *a*, *c* green, and *C*, *A*, *b*, *d* blue.
6. *B*, *C*, *a*, *d* green, and *A*, *D*, *b*, *c* blue.

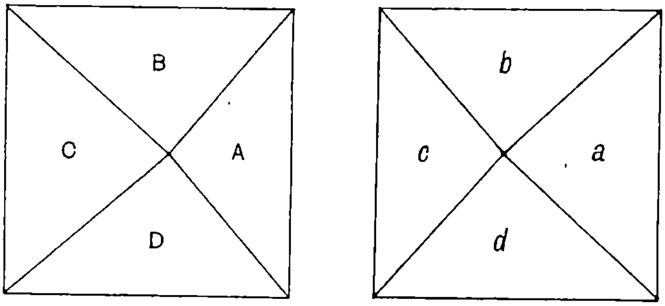

Fig. 2.

The sides of the squares were each 38 mm. and the apex of the pyramid was 23 mm. from the right side of the figure on the right and from the left side of the figure on the left. The disparity was made sufficiently great to ensure the production of a very pronounced stereoscopic effect which could not easily be confused with an illusory effect and could readily be destroyed by various conditions, such as fixation and difference of coloration.

Other slides were made by colouring the triangles with water-colours, but they were not so satisfactory, because it was very difficult to spread the colour evenly: the grain of the paper caused it to lie in minute pools. Coloured papers are preferable also because they can be found in any laboratory, and thus readily permit repetition of the experiment.

As the results of these experiments are of more interest for theories of the perception of solidity than for theories of binocular colour mixture, I shall not attempt to report them fully here, but shall briefly indicate the effect of these slightly disparate contours on colour combination.

The results given by different subjects differ greatly, but there are some points of agreement. The stereoscopic effect appears most quickly and most clearly in those slides in which corresponding sides of the pyramid are similarly coloured (slides 1, 3 and 4). With slides in which the corresponding sides are differently coloured there is often some difficulty in getting the stereoscopic effect, and it is never very stable: it appears more readily in slide 2 than in slide 6, and in slide 6 than in slide 5.

The clearer the stereoscopic effect, the steadier is the colour of the figure; hence mixture takes place more readily in slide 2 than in slide 6, and a little more readily in 6 than in 5. As mixture is produced a little more readily in slide 2 than in one in which there are no contours within the coloured areas, the stereoscopic figures favour mixture.

When the stereoscopic effect appears in slides of which corresponding parts are dissimilarly coloured (Nos. 2, 5, 6), the colour of the pyramid is never exactly like either of the uniocular colours. It is often quite steady, but sometimes fluctuates. Whenever the pure uniocular colours appear, the solidity disappears. Colours of different brightness give to the pyramid a 'shot' or lustrous appearance: this is especially noticeable in slides like No. 5, in which adjacent sides of the same figure are differently coloured.

Fixation destroys both the stereoscopic effect and binocular colour mixture. The greater the difference between the colours of corresponding parts, the more difficult is it to get steady solidity and colour mixture. Unsaturated colours mix readily. The colours that differ most from the background tend to predominate. As in all the experiments already reported, so here also it was found that some subjects got mixture more frequently than others.

Experiments with coloured glasses have been described so often that I shall not attempt to give an account of my own: they show that slightly disparate contours do to some extent facilitate mixture. There are, however, two features of these experiments that are worthy of attention.

Grey glasses give less rivalry than coloured glasses of similar brightness. Rivalry is seldom detected when a grey glass is held before one eye unless the glass be very dark and some part of the field be fixated, whereas when a glass of well-marked colour is held before one eye, there is almost invariably clear rivalry.

Another point of interest in the results of experiments with grey glasses is the complete absence of lustre in the binocular field. Lustre appears occasionally when coloured objects are observed through coloured glasses, but only if these make a small portion of one uniocular image darker relatively to the rest of the field than it is in the other image. A square of red paper, seen by one who is holding a red glass before one eye and a green glass before the other, is lustrous while the background is dull. Here the conditions approximate to those of the experiments described on pages 528–9, for the red paper seen through the red glass alone is quite or almost as bright as the background, whereas seen through the green glass it is very much darker.

As no one seems to have attempted to compare the darkening of the binocular field which is produced by holding a grey glass before one eye with the darkening produced by dividing the illumination equally between the two eyes, it may be advisable here to describe some experiments made for the purpose of making this comparison. I used both grey glasses and an episcotister, and found the latter preferable because it permitted great variation and accurate measurement of luminosities. Two of these instruments were set up: through one (A) the observer looked with one eye at various objects, the other eye being uncovered; through the other (B) he looked with both eyes at the same objects. The sectors of the instruments were adjusted so that the light transmitted by the second (B) was the mean of the amount transmitted by the other (A) and the amount entering the uncovered eye, *e.g.* if A transmitted m % of the light which fell on it, B transmitted $\frac{m+100}{2}$ %. The binocular field given by A and the uncovered eye corresponded to the binocular mixture of the upper discs used in the experiments described on pages 534–6, and the field given by B corresponded to the uniocular mixture of the lower discs. The comparison of the two fields had to be made successively, and was therefore not made so easily as in those experiments.

Until the region of maximal binocular darkening was reached, the binocular field given by A and the uncovered eye was equal or approximately equal in brightness to the corresponding uniocular mixture (that seen through B). As I used light reflected from a sheet of white paper, my light-intensities were very much lower than those of Fechner, who used the illumination of a bright sky: this probably accounts for the fact that whereas Fechner [(18), p. 448] found the region of maximal darkening to be given by glasses which transmitted about 4 % of the light that fell on them, it was given in my experiments by rotating sectors which transmitted from 12 to 16 %. Like Aubert, I found that the minimum-point on the curve representing the brightnesses of the binocular field varied with the intensity of the illumination.

The introspection in this region is interesting for the evidence it affords of the influence of contours. With luminosities greater than those which gave maximal darkening of the binocular field the paradoxical effect appeared immediately, with lower luminosities there was an obscuration rather than a darkening of the field; it was as if something was present which prevented the subject from seeing clearly, and was quite different from any degree of darkening given by greater luminosities. In the region of maximal darkening the appearance of the binocular field depended on the duration of the observation. At first the lower luminosities produced merely a slight obscuration of the field, but after a time, when presumably the darkened eye had adapted itself to the reduced illumination, there appeared a pronounced darkening of the binocular field. The time required for such adaptation varied with the amount of light transmitted by the episcotister and with the condition of the eye.

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PUBLICATIONS RECEIVED

(1) *La Misura dell' Intelligenza nei Fanciulli.* By F. UMBERTO SAFFIOTTI. Lire 6. Roma: 1916. Pp. viii + 286.

(2) *La "Scala Metrica dell' Intelligenza" di Binet e Simon.* By ZACCARIA TREVES E F. UMBERTO SAFFIOTTI. Milano: Stabilimento Tipolitografico G. Cavelli, 1911. Pp. 67.

(3) "*L'Echelle Metrique de l'Intelligence de Binet-Simon modifiée, selon la Méthode Treves-Saffiotti.*" By F. UMBERTO SAFFIOTTI. *L'Année Psychologique,* Tome XVIII. Paris: Masson et Cie, 1913. Pp. 14.

(4) "*Contributo allo Studio dei Rapporti tra l'Intelligenza e i Fattori Biologico-sociali.*" By F. UMBERTO SAFFIOTTI. Estratto dalla *Rivista di Antropologia,* vol. XVIII, Fasc. i–ii. Roma: Presso la Sede della Società, 1913. Pp. 34.

(5) "*Note Psicologice' su du Gemelle.*" By F. UMBERTO SAFFIOTTI. Estratto dalla *Rivista di Antropologia,* vol. XIX, Fasc. iii. Roma: Presso la Sede della Società, 1913. Pp. 11.

(6) "*Sul Quoziente d' Intelligenza.*" By F. UMBERTO SAFFIOTTI. Estratto dalla *Rivista di Antropologia,* vol. XIX, Fasc. iii. Roma: Presso la Sede della Società, 1914. Pp. 10.

Together these reprints and this volume form a most valuable accession to the literature dealing with the testing of intelligence.

Professor Saffiotti's book (1) is divided into three parts. The first deals with the general determination of intelligence in its relations to physical and mental characteristics. Under four or five main categories the author briefly reviews preceding researches and leads up to a discussion of the Binet Scale. The method of measuring intelligence by mental age is criticised in the second part of the book. Many of the objections raised agree with those previously expressed by investigators in this country: and, since the critics of the Binet scale have, until recently, been comparatively few, it is of special interest to find the earlier criticisms thus independently confirmed upon an unusually wide basis of experimental results.

The third part describes the method of measurement by "mental grades." This method involves two fundamental modifications of Binet's original proposal. First, instead of applying tests simply for age, the tests are re-grouped according to both age and school-classes, and then classified, upon an empirical base, into tests too hard, hard, medium, easy, and too easy, for each age-class group.

Results obtained by testing nearly 1,000 Milanese children are reported: and the new method of estimation is shown to give a far truer picture of the distribution of their ability than the Binet-Simon estimate. Secondly, instead of designating the child's level of intelligence in terms of 'years,' the examinee is assigned to one of three grades—'deboli,' 'medii,' 'forte'—and if necessary, to one of the three subgrades within each grade, (deboli-deboli, medii-deboli, forte-deboli, etc.,) according as he passes the 'hard,' medium, or easy tests of his age or class.

(2) The joint publication with he late Professor Treves gives an Italian translation of the tests, and detailed diagrams showing the actual order of difficulty furnished by the experiments as contrasted with that drawn up by Binet. The general conclusion reached by the author is here concisely summarised. "*Il materiale proposto da B. e S. è un materiale utilizzabile, ma mal distributo,* al meno per la nostra populazione scolastica."

Binet's attitude towards this criticism was characteristic. "Your conclusions," he wrote, "are very severe. No matter. I congratulate you on your thorough piece of work." And he invited Professor Saffiotti to summarise the Treves-Saffiotti modifications in an article for his Journal. The French pamphlet (3) gives a brief but clear summary of the earlier proposals of the two Italian critics.

The data derived from the four Milanese schools have been analysed according to the occupations and professions followed by the parents of the children tested. The percentages of dull, mediocre, and bright children in each social group have been calculated, and mental indices for the general groups elaborated. The professional and commercial classes appear to have the highest index: tradespeople and artisans, the lowest; servants occupy an intermediate position.

The remaining pamphlets contain a brief study of a pair of twins by means of the revised scheme of tests (5) and a detailed criticism of Stern's suggestion to substitute an "Intelligence-quotient" for Binet's original measure of retardation.

The space allotted in this *Journal* of books received forbids an adequate notice of these suggestive publications. Those to whom the Italian writings are inaccessible will find the suggestive innovations of Professors Treves and Saffiotti described and discussed more fully in the current number of the *Eugenics Review*[1].

Psychologie de l'Enfant et Pédagogie Expérimentale. Par Prof. ED. CLAPARÈDE. *Cinquième édition, revue et augmentée.* Genève: Libraire Kundig, 1916. Pp. xii + 571. Prix 8 fs.

The appearance of the fifth edition of this interesting volume bears witness to the very considerable amount of attention that is now being given to the experimental treatment of problems of education. Dr Claparède has thoroughly revised his work, and has added a number of sections. The *Introduction* furnishes a lively and convincing reply to those who maintain that practical education has nothing important to learn from scientific child study. Following this is an *Aperçu historique,* which has been brought completely up to date, while the sections on *Problèmes* and *Méthodes* have been greatly extended, a fairly lengthy description of biometrical and statistical methods having been added. The concluding section, on *Le Developpement Mental,* is largely concerned with the nature and function of Play in the development of the life of the individual. There is also an interesting discussion of the conception of Interest. The section on Mental Fatigue, which appeared in earlier editions of this work, has now been dropped, and is to form part of projected volumes which will deal with questions of memory, of thinking, and of character.

The lists of further authorities to be consulted have been brought up to date, and form a useful bibliography.

Perhaps a little more discrimination might have been shown in the sifting of the good from the bad in the large amount of work that is described. The survey, too, of the results of investigations is hardly as comprehensive as his survey of methods: and in the chapter upon mental development a disproportionate amount of space is perhaps given to the topic of Play. But, as a whole, the book is heartily to be recommended to any who wish to get a general view of the subject. It is written throughout in a lively and interesting manner, and displays much breadth of view, and clearness of exposition.

The Relation of General Intelligence to Certain Mental and Physical Traits. By CYRUS D. MEAD, Ph.D., Teacher's College, Columbia University. Contributions to Education, No. 76. $1.50. New York City: Teacher's College, Columbia, 1916. Pp. 117.

The author compares 480 normal and 430 feebleminded children in regard to age of walking and talking, height and weight, strength of grip and dextrality, perception (erasure of a letter) and memory for related and unrelated words.

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[1] *Eugenics Review,* Vol. VIII, no. 4, pp. 364—373. It is unfortunate that in English-speaking countries recent Italian work in psychology has not attracted the attention given to earlier studies associated with the names of Lombroso and his contemporaries.

The normal walks at 13·5 ± 1·1 months: the defective at 21·6 ± 7·6 months. The normal talks at 15·8 ± 2·8 months: the defective at 34·4 ± 12·8 months.

Unfortunately while the data for normals was taken to the nearest month the data for defectives was taken to the nearest half-year: hence, a measure of overlap is hardly calculable. The normal group, too, was rather small—25 of either sex—and consisted of children of somewhat superior social and intellectual status. The informants evidently had the usual predilections for round, whole, or even numbers. Hence the ranges, especially for the normals, are probably too small.

The conclusions for height and weight, on the whole, confirm those of Goddard: those for 'perception' and 'memory' those of Miss Norsworthy. Defectives are shorter and lighter than normals: and the greater the mental deficiency, the greater appears the physical deviation. Defectives are poorer than normals in 'perception' and 'memory': but they do better in these respects than in any of the more purely mental traits.

Defectives have also a weaker grip than normals, the divergence increasing with age and degree of defect; the difference between the right and left hand is less marked among defectives.

The writer tabulates the individual records of these and other tests in full: and appends detailed references upon each topic.

His chief results may be summarised as follows in terms of the percentage of defectives reaching or exceeding the median of the normal group of the same age and sex.

	Boys	Girls		Boys	Girls
Height ...	20·7	20·8	'Perception' ...	5·5	4·0
Weight ...	29·6	41·2	Memory		
Strength of Grip			Related words	8·5	7·3
Right hand	17·8	—	Unrelated words	12·4	13·1
Left hand ...	22·3	—			

Completion-Test Language Scales. By MARION REX TRABUE, Teacher's College, Columbia University. Contributions to Education, No. 77. New York City, 1916. Pp. 118. $1.50.

This monograph describes an attempt to derive scales for the measurement of ability to complete sentences. Ebbinghaus' completion-test has been variously described as a test of intelligence, of imagination, association, memory, and numerous other capacities. The present author takes it as a measure of capacity very closely related to what is ordinarily termed language ability.

The author began by testing between 800 and 900 children with a tentative list of 56 completion test sentences. On the basis of these preliminary experiments the list was radically revised and the sentences rearranged in order of difficulty. With this new graded series over 5,000 children were tested in the public schools of New York and New Jersey. By means of these results two sentences were selected for each school grade, namely those which proved to be difficult enough to be done by about 50 per cent. in that grade. This list forms the author's main scale called "Language Scale A."

With "Scale A" some 6,000 children chiefly from schools of New York and New Jersey were tested. The distribution of the scores made by all the children tested, some 10,062, are presented summarily in tables. The intervals between the various grades prove to be somewhat unequal: but this is apparently due to the unequal steps in the grading of the scale. Conformably with general opinion as regards sex differences in language ability, the boys make a somewhat lower median score than girls of the same grade. But the difference is small and the amount of overlapping is enormous.

The variability of the grades is smaller than that of the age-groups. For example, that of the sixth grade is only 73 per cent. of that of the 12-year-old age-group. Apparently, to say that a child is 12 years old is saying far less concerning the child's ability to complete sentences than saying he is in the sixth grade.

In a brief experiment the author illustrates how his scale may be applied to test improvement from year to year. Eight other additional scales have also been drawn up so arranged that the intervals between the steps are more nearly equal. The difficulty of the test-sentences is based upon the probable error of the grades. The methods of calculation are those familiarised by Buckingham's monograph upon spelling ability.

An appendix gives correlations of the completion-test with the results of the Binet and other scales and detailed schemes for scoring.

Measurements of Some Achievements in Arithmetic. By CLIFFORD WOODY, Ph.D., Teacher's College, Columbia University. Contributions to Education, No. 80. New York City: Teacher's College, Columbia University, 1916. Pp. 63. $1.00.

This monograph gives the results of an endeavour to derive a series of scales in the fundamental operations of arithmetic. The method employed closely follows that adopted by Buckingham in developing a spelling scale and Trabue in developing completion-test scales. Each scale is composed of as great a variety of problems as possible. Each begins with the easiest problem conceivable, and increases progressively in difficulty as far as problems, which only a small percentage of eighth grade pupils can perform.

Two final series have thus been derived. Large numbers of problems were originally used: and given to nearly 1,000 pupils. The revised sheets were set by the writer personally to about 4,000 pupils in seven different school systems. An attempt has been made to measure the difficulty of the various problems according to the percentage of children answering them. The probable errors of the grades are taken as the unit of measurement: and normal distribution assumed,—perhaps not quite legitimately. Ingenious methods are employed for measuring the distance between the grades in terms commensurable with their probable errors, and for referring all the problems to an arbitrary zero point.

In the second scale the problems are so selected as to form a linear scale with equal steps between each successive pair. For both scales,—problems, answers, instructions, and tentative standards of achievement are given in detail: and a page or two is added on the uses and limitations of the scales.

Adjustment of School Organisation to Various Population Groups. By ROBERT A. F. MCDONALD, Ph.D. Contributions to Education, No. 75. New York City: Teacher's College, Columbia University, 1915. Pp. 145. $1.50.

Professor McDonald reviews from a historical standpoint the provision, private and public, made in the United States for the special types of school children, deaf, blind, cripples, epileptic, feebleminded, truant, delinquent, foreign-speaking, neglected, defective in speech, and exceptionally gifted. In an appendix he gives lists of private and public institutions, schools and classes, catering for these children, together with the date of opening.

The progress of each movement is clearly illustrated by graphs. His figures and diagrams show plainly how "sympathy operates more immediately in the case of the unfortunate....There are more readily discernible types of subnormality than types of superiority." Special classes for the deaf, the blind, and the orphaned were among the first established. Less marked though more frequent cases of subnormality—the tuberculous, and those with defective speech—these attracted notice later. Those exhibiting unusual ability were not definitely selected and classified for special teaching until late in the nineteenth century. The writer quotes freely to illustrate the practical influence which the work of the psychologist has had in furthering these movements, more particularly in the case of the mentally defective and the exceptionally gifted.

To each chapter dealing with the several forms of special educational provision is appended a valuable bibliography.

Anthropometry as an Aid to Mental Diagnosis. By E. A. DOLL. Vineland, New Jersey: Publications of the Training School, No. 8, 1916. 75 c.

This publication of the Vineland Research Department contains a most thorough and suggestive analysis of physical and 'psycho-physical' measurements taken from the anthropometric files of the laboratory of the Training School. 477 children have been measured and re-measured by the author and by research students for six characteristics—height, standing and sitting, weight, grip with right and left hand, and 'vital capacity.' By means of Smedley's tables, here reprinted in full, the original measures are converted into percentiles. Correlations are worked out between each measurement and the rest, and between each measurement and mental age. The last three measurements,—the so-called 'psycho-physical'—yield a higher correlation with mental age than the first three,—the merely 'physical,'—namely, on an average ·68 as against ·37. The following are the percentages of the defectives reaching the median figure obtained at each age by Smedley with normal children:

	Height Standing	Height Sitting	Weight	Average for 'Physical' Measurements	Grip R. H.	Grip L. H.	Vital Capacity	Average for 'Psycho-physical' Measurements
Boys	34	26	39	32	9	13	8	7
Girls	30	24	46	29	11	18	7	7

Low percentiles for psycho-physical measurements are thus diagnostic both of mental deficiency as distinguished from normality and of low mental grade or age as distinguished from a high one. The variability of the groups, however, is very high, especially for the single measurements: and this somewhat detracts from their diagnostic value. An excess of average percentile for physical measurements over the average for psycho-physical is characteristic of mental deficiency. The absolute ratio of the two averages is said to yield a reliable diagnosis for rapid purposes. Sample charts are given exhibiting the general trend of the measurements graphically, somewhat after the manner of the 'psychographs' employed by investigators in this country.

Incidentally, many important results are reached, confirming, in the case of height and weight, the recent work of Goddard and of Mead. The original measurements are printed in full: and the monograph thus includes a valuable collection of data. We are especially grateful to have the Smedley tables, which were unfortunately out of print, here reprinted in an accessible form.

The Freudian Wish and its Place in Ethics. By EDWIN B. HOLT. London: T. Fisher Unwin, Ltd., 1915. Pp. x + 212.

The psycho-analytic movement has hitherto developed in a manner so independent of academic psychology that its relations to other branches of mental science are in many respects far from being clear at the present time. All the greater interest attaches to the present volume which endeavours to indicate some of the relations of Freud's work to the problems of ethics and of behaviouristic psychology.

The work falls naturally into three parts. In the first of these (Chapter I. on "The Doctrine of the Wish") there is an exposition of some of the main principles of Freud's psychology, with fairly numerous illustrations from dreams, wit and psycho-pathology of everyday life. There is but little attempt to grapple with the deeper aspects of the problems under consideration, but for the reader who is unacquainted with Freud's work the treatment is illuminating so far as it goes.

In the second part (which includes Chapter II. on "The Physiology of Wishes," and a somewhat lengthy appendix on "Response and Cognition," originally published as an article in the *Journal of Philosophy, Psychology and Scientific Methods*, Vol. XII.) we find an interesting statement of the author's own position as a behaviourist and an endeavour to show the relations of Freud's psychology to this position. Behaviour or "specific response" (we are given to understand) arises from the interaction of reflexes and becomes gradually more complex as the integration of the reflexes becomes higher. The Freudian "wish" is identical with specific response and, understood in this way, should be regarded by the psychologist as the ultimate mental unit, replacing the subjective unit of sensation. The mere introspective study of consciousness is of no assistance in the understanding of conduct, since "consciousness is not a substance but a relation—the relation between the living organism and the environment to which it specifically responds." To understand behaviour we must ask, What is the organism doing? When we obtain an answer to this question, we arrive at the "wish" which underlies the actions of the organism. In all this the author has only made a very superficial use of Freud, and is indeed for the most part more closely in touch with Sherrington and (to some extent) with Spencer than with the Viennese psychologist.

The majority of readers will probably derive most satisfaction from the third portion of the book, comprising Chapters III. and IV., of which Chapter III. contains the main thesis dealing with "The Wish in Ethics." Here, as to some extent in the earlier part, the principal conception is that of integration. With the help of a number of interesting and entertaining examples it is shown how the most moral conduct, in situations giving rise to conflicts of an ethical nature, is achieved only by a full understanding and appreciation of the nature of the conflicting tendencies and by the integration of the tendencies which is thereby made possible. This is opposed to the method of Repression (or, as Prof. Holt calls it, Suppression) which leads to dissociation, prejudice, one sided and inharmonious development and greatly increased difficulty in bringing about a satisfactory adjustment of the individual to his environment. Freud's discoveries obviously lend themselves to ethical interpretation on these lines, and what Prof. Holt has to say on the subject combines excellent moral teaching with sound common sense. We may regret however that here, as in the first section, the author's treatment remains throughout at a relatively superficial level, no attempt being made to follow Freud into those deeper problems of human personality to which the method of psycho-analysis has opened up the way. There is, for instance, no indication of the relative importance of the part played in moral conflicts by conscious and unconscious tendencies respectively, though the distinction between conscious and unconscious motives is, we should have thought, of primary importance for any application of Freud's psychology to ethics. Again, although the book is concerned to a considerable extent with the concept of moral development, there is nowhere any appreciation of the infantile residua which Freud has shown to be operative in adult mental life; nor is there any reference to the processes of Displacement and Sublimation, which are surely of the highest importance in mental and moral growth. Even as regards the purely conscious motives of which account is taken, Prof. Holt probably tends to over-emphasize the importance of the intellectual and to undervalue that of the affective and emotional factors concerned—a course which is perhaps the more surprising after the emphasis laid upon the "wish" and the unimportant rôle attributed to conscious thought in the earlier part of the book. In the rather undue importance which, in the later chapters, would seem to be attached to mere knowledge, we may perhaps trace the influence of Breuer and Freud's "cathartic" treatment rather than that of the subsequently developed "psycho-analytic" procedure.

It will be seen that the present study is very far from presenting a full account of the relations of Freud's psychology to behaviourism or to ethics. Indeed the relatively modest dimensions of the book indicate that it has in view no such exhaustive treatment of its subject. In spite of a certain narrowness of scope however, the book will repay a careful study by revealing many avenues of interesting thought and speculation and will undoubtedly (and deservedly) be read with pleasure and profit by many different kinds of readers. Its main thesis on the ethical side is, moreover, almost certainly, both correct and of very considerable importance.

J. C. F.

Mind, Nos. 100, 101, 102. London: Macmillan & Co., 1916, 1917. 4*s*.

The Philosophical Review, Vol. XXVI. Nos. 1 and 2. New York: Longmans, Green & Co., 1917. $3.00 per annum.